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INDEX TO *ECONOMICA*

NEW SERIES, VOLUME XI, 1944 (NUMBERS 41–44)

AUTHORS

Bauer (P. T.) Some Aspects of the Malayan Rubber Slump 1929–1933. p. 190.
Beacham (A.) The Ulster Linen Industry. p. 199.
Boulding (K. E.) A Liquidity Preference Theory of Market Prices. p. 55.
Garvy (George) Dr. Rhodes' Analysis of the Distribution of Single Incomes in the United States. p. 104.
Hayek (F. A. v.) Scientism and the Study of Society. Part III. p. 27.
Horsefield (J. K.) The Duties of a Banker—II. p. 74.
Horsefield (J. K.) The Origins of the Bank Charter Act, 1844. p. 180.
Lacey (K.) Commodity Stocks and the Trade Cycle. p. 12.
Lacey (K.) A Reply [to Mr. Harry Norris's Note]. p. 140.
Lachmann (L. M.) Finance Capitalism ? p. 64.
Lutz (F. A.) History and Theory in Economics. p. 210.
Morgan (E. Victor) The Joint Statement by Experts on the Establishment of an International Monetary Fund. p. 112.
Norris (Harry) A Note [to Mr. K. Lacey's article]. p. 138.
Popper (Karl) The Poverty of Historicism, Part I. p. 86. Part II, p. 119.
Rhodes (E. C.) Comments on Dr. Garvy's Note. p. 106.
Rhodes (E. C.) The Pareto Distribution of Incomes. p. 1.
Rothschild (K. W.) Public Expenditure in the National Income: A Note. p. 19.
Stern (Ernest H.) The Agreements of Bretton Woods. p. 165.
Stern (Ernest H.) Public Expenditure in the National Income: A Reply. p. 23.
Whale (P. Barrett) A Retrospective View of the Bank Charter Act of 1844. p. 109.

LIST OF TITLES

The Agreements of Bretton Woods; by Ernest H. Stern. p. 165.
Comments on Dr. Garvy's Note; by E. C. Rhodes. p. 106.
Commodity Stocks and the Trade Cycle; by K. Lacey. p. 12.
—— A Note; by Harry Norris. p. 138.
—— A Reply; by K. Lacey. p. 140.
Dr. Rhodes' Analysis of the Distribution of Single Incomes in the United States; by George Garvy. p. 104.
The Duties of a Banker—II; by J. K. Horsefield. p. 74.
Finance Capitalism ? by L. M. Lachmann. p. 64.
History and Theory in Economics; by F. A. Lutz. p. 210.
The Joint Statement by Experts on the Establishment of an International Monetary Fund; by E. Victor Morgan. p. 112.
A Liquidity Preference Theory of Market Prices; by K. E. Boulding. p. 55.
The Origins of the Bank Charter Act, 1844; by J. K. Horsefield. p. 180.
The Pareto Distribution of Incomes; by E. C. Rhodes. p. 1.
The Poverty of Historicism; by Karl Popper. Part I. p. 86. Part II. p. 119.
Public Expenditure in the National Income. A Note; by K. W. Rothschild. p. 19.
—— A Reply; by Ernest H. Stern. p. 23.
A Retrospective View of the Bank Charter Act of 1844; by P. Barrett Whale. p. 109.
Scientism and the Study of Society. Part III; by F. A. v. Hayek. p. 27.
Some Aspects of the Malayan Rubber Slump 1929–1933; by P. T. Bauer. p. 190.
The Ulster Linen Industry; by A. Beacham. p. 199.

SUBJECTS

Economics, Applied. Bauer (P. T.) Some Aspects of the Malayan Rubber Slump 1929–1933. p. 190.
—— Beacham (A.) The Ulster Linen Industry. p. 199.
—— Horsefield (J. K.) The Duties of a Banker—II. p. 74.
—— Lacey (K.) Commodity Stocks and the Trade Cycle. p. 12.
—— Lacey (K.) A Reply [to Mr. Harry Norris's Note]. p. 140.
—— Morgan (E. Victor) The Joint Statement by Experts on the Establishment of an International Monetary Fund. p. 112.
—— Norris (Harry) A Note [to Mr. K. Lacey's article]. p. 138.
—— Rothschild (K. W.) Public Expenditure in the National Income: A Note. p. 19.
—— Stern (Ernest H.) The Agreements of Bretton Woods. p. 165.
—— Stern (Ernest H.) Public Expenditure in the National Income: A Reply. p. 23.
Economics: Pure Theory. Boulding (K. E.) A Liquidity Preference Theory of Market Prices. p. 55.
—— Lachmann (L. M.) Finance Capitalism? p. 64.
Economic History. Horsefield (J. K.) The Origins of the Bank Charter Act, 1844. p. 180.
—— Whale (P. Barrett) A Retrospective View of the Bank Charter Act of 1844. p. 109.
Statistics. Garvy (George) Dr. Rhodes' Analysis of the Distribution of Single Incomes in the United States. p. 104.
—— Rhodes (E. C.) Comments on Dr. Garvy's Note. p. 106.
—— Rhodes (E. C.) The Pareto Distribution of Incomes. p. 1.
General. Hayek (F. A. v.) Scientism and the Study of Society. Part III. p. 27.
—— Lutz (F. A.) History and Theory in Economics. p. 210.
—— Popper (Karl) The Poverty of Historicism. Part I. p. 86. Part II. p. 119.

REVIEWS

Batson (E.) *A Re-Calculation of the Poverty Datum Line.* S. N. p. 162.
Benham (F.) *Economics.* By C. W. Guillebaud. p. 51.
Benham (F.) *The National Income of Jamaica 1942. The National Income of Barbados 1942.* By R. Stone. p. 216.
Berry (T. S.) *Western Prices Before 1861.* By M. J. Elsas. p. 148.
Brimble (L. J. F.) and May (F. J.) *Social Studies and World Citizenship.* S. N. p. 162.
Burnham (T. H.) and Hoskins (G. O.) *Iron and Steel in Britain 1870–1930.* By Allan G. B. Fisher. p. 143.
Caillet (E.) *La tradition littéraire des idéologues.* By F. A. v. Hayek. p. 158.
Condliffe (J. B.) *The Economic Pattern of World Population.* S. N. p. 52.
Coupland (R.) *Report on the Constitutional Problem of India.* By Vera Anstey. p. 48.
The Economist 1843–1943. By F. A. v. Hayek. p. 51.
Eucken (W.) Die Grundlagen der Notionaloekonomie. By F. A. Lutz. p. 210.
Faville (D. E.), Jones (D. M.) and Sonne (R. B.) *Merchandise Availability in Utah.* S. N. p. 53.
Fellner (W.) *A Treatise on War Inflation.* By J. K. Horsefield. p. 42.
Foenander (O. De R.) *Wartime Labour Developments in Australia.* By Allan G. B. Fisher. p. 108.
Freeman (J. B.) and Hignett (N. H.) *Commodity Control.* By Vera Anstey. p. 48.
Gutkind (E. A.) *Creative Demobilisation.* By A. L. Macfie. p. 46.
Hailey, Lord. *The Future of Colonial Peoples.* By Charlotte Leubuscher. p. 155.

Hallowell (J. H.) *The Decline of Liberalism as an Ideology.* By F. A. v. Hayek. p. 159.

Hamburger (L.) *How Nazi Germany has Controlled Business.* By H. W. Singer. p. 218.

Hayek (F. A.) *The Road to Serfdom.* By C. W. Guillebaud. p. 215.

Hobson (O. R.) *Can We Afford It?* By F. A. v. Hayek. p. 52.

Hower (R. M.) *History of Macy's of New York, 1858–1919.* By Bernice Holt-Smith. p. 147.

Hultgren (T.) *Railway Traffic Expansion and Use of Resources in World War II.* S. N. p. 162.

Kalecki (M.) *Studies in Economic Dynamics.* By G. L. S. Shackle. p. 217.

Kuznets (Simon) *National Product: War and Pre-War.* S. N. p. 162.

Lauterbach (A. T.) *Economics in Uniform: Military Economy and Social Structure.* By E. Rosenbaum. p. 43.

League of Nations. *Agricultural Production in Continental Europe during the 1914–18 War and the Reconstruction Period.* By Charlotte Leubuscher. p. 155.

League of Nations. *Economic Fluctuations in the United States and the United Kingdom, 1918–22.* By R. G. Hawtrey. p. 107.

League of Nations. *Europe's Overseas Needs 1919–20 and How They Were Met.* By Vera Anstey. p. 151.

League of Nations. *Relief Deliveries and Relief Loans, 1919–23.* S. N. p. 162.

League of Nations. *Trade Relations Between Free Market and Controlled Economies.* By Vera Anstey. p. 108.

League of Nations. *The Transition from War to Peace Economy.* By Vera Anstey. p. 152.

Mather (K.) *Statistical Analysis and Biology.* By E. Grebenik. p. 160.

Megrah (M.) *The Bills of Exchange Act, 1882.* By O. Kahn-Freund. p. 47.

Mendershausen (H.) *The Economics of War.* By J. K. Horsefield. p. 147.

Moore (G. H.) *Production of Industrial Materials in World Wars I and II.* S. N. p. 162.

Morton (W. A.) *British Finance 1930–1940.* By J. K. Horsefield. p. 145.

Mukerjee (R.) *The Institutional Theory of Economics.* By Vera Anstey. p. 45.

Naidu (B. V. N.) and Thiruvengadathan (S.) *The Madras General Sales Tax Act.* S. N. p. 162.

Nathan (O.) *The Nazi Economic System.* By H. W. Singer. p. 218.

Normano (J. F.) *The Spirit of American Economics.* By F. A. v. Hayek. p. 159.

Notestein (F. W.) *The Future Population of Europe and the Soviet Union. Population Projections 1940–1970.* By E. Grebenik. p. 161.

Pritchett (C. H.) *The Tennessee Valley Authority.* S. N. p. 162.

Report of the Foodgrains Policy Committee. (India). By Vera Anstey. p. 144.

Research Staff of the N.I.E.S.R. *Trade Regulations and Commercial Policy of the United Kingdom.* By Vera Anstey. p. 50.

La Revue Economique et Sociale. By F. A. v. Hayek. p. 161.

Roepke (W.) *Die Gesellschaftskrisis der Gegenwart.* By A. Shenfield. p. 151.

Schumpeter (J.) *Capitalism, Socialism and Democracy.* By R. G. Hawtrey. p. 40.

Shoup (C.), Friedman (M.) and Mack (Ruth P.) *Taxing to Prevent Inflation.* By J. K. Horsefield. p. 153.

Simutis (Anicetas) *The Economic Reconstruction of Lithuania after 1918.* S. N. p. 52.

Singer (H. W.) *Standardised Accountancy in Germany.* By Basil Smallpeice. p. 45.

Slesser (Sir H.) *A History of the Liberal Party.* By F. A. v. Hayek. p. 157.

Smith (Wilfred) *The Distribution of Population and the Location of Industry on Merseyside.* S. N. p. 53.

Sovani (N. V.) *The Population Problem in India: A Regional Approach.* By Vera Anstey. p. 156.

Subramanian (S.) *India. Guide to Current Official Statistics.* By Vera Anstey. p. 50.
Tinley (J. N.) *The Native Labor Problem of South Africa.* By Sheila T. van der Horst. p. 149.
Tippett (L. H. C.) *Statistics.* By E. Grebenik. p. 160.
Titmuss (R. M.) *Birth, Poverty and Wealth.* By A. L. Bowley. p. 143.
Tiwari (R. D.) *Modern Commercial Policy: A Study in Technique.* By Vera Anstey. . p. 157.
Walker (E. R.) *From Economic Theory to Policy.* By J. E. Meade. p. 41.
Woytinsky (W. S.) *Earnings and Social Security in the United States.* By Brinley Thomas. p. 154.
Young (M.) and Bunbury (H. N.) *Will the War Make Us Poorer?* S. N. p. 52.

AUTHORS OF REVIEWS

Vera Anstey. 45, 48, 50, 108, 144, 151, 152, 156, 157.
A. L. Bowley. 143.
M. J. Elsas. 148.
Allan G. B. Fisher. 108, 143.
E. Grebenik. 160, 161.
C. W. Guillebaud. 51, 215.
F. A. v. Hayek. 51, 52, 157, 158, 159, 161.
R. G. Hawtrey. 40, 107.
Bernice Holt-Smith. 147.
J. K. Horsefield. 42, 145, 147, 153.
Sheila T. van der Horst. 149.
O. Kahn-Freund. 47.
Charlotte Leubuscher. 155.
F. A. Lutz. 210.
A. L. Macfie. 46.
J. E. Meade. 41.
E. Rosenbaum. 43.
G. L. S. Shackle. 217.
A. Shenfield. 151.
H. W. Singer. 218.
Basil Smallpeice. 45.
R. Stone. 216.
Brinley Thomas. 154.

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The Pareto Distribution of Incomes

By E. C. Rhodes

Part I

Let us suppose that a population consists of a number of groups of persons distinguished by the possession of talents, which, being exercised, enable their possessors to contribute to the total output of the population and to share the income. For simplicity we will suppose those in the first group to possess one and the same talent, those in the second group to possess two talents, those in the third group to possess three, and so on. We will assume that the numbers in these groups progressively decline, there being most in the first group, fewer in the second group, fewer still in the third group and so on. We will assume that the person with only one talent produces, on the average, less than the person with two talents; similarly the person with two talents produces, on the average, less than the person with three, and so on. Similarly, we will suppose that, on the average, the consequent income increases with the possession of more and more talents. But, although we assume the persons in each group to possess the same number of talents, we assume variation within the group in respect of the income accruing. We will assume that all the groups have the same coefficient of variation for income, i.e., the ratio of the standard deviation to the average of the group is constant for all the groups. We will further assume that the type of distribution of income is the same for all groups. In the numerical example which follows we assume the normal distribution. We can set out our assumptions in the following scheme:—

	Groups					
Number of talents ..	1	2	3	4	5	. . .
Number of persons ..	p_1	p_2	p_3	p_4	p_5	. . .
Average income	m_1	m_2	m_3	m_4	m_5	. . .
Standard Deviation	λm_1	λm_2	λm_3	λm_4	λm_5	. . .
Distribution	Normal					

Our assumptions involve $p_1 > p_2 > p_3 > \ldots$, $m_1 < m_2 < m_3 < \ldots$; λ is the coefficient of variation.

Let us take a concrete example from which we can compute the distribution of income for the whole population. We will assume $p_r = \frac{1,000,000}{3^{r-1}}$, thus the p's form a geometrical progression. We will assume $m_r = 2^{r-1}m$, so that the m's also form a geometrical progression. We shall have $m_{r+1} - m_r = 2^r m - 2^{r-1}m = 2^{r-1}m$. Thus, if we assume $\lambda = \frac{1}{3}$, $\frac{m_{r+1} - m_r}{\lambda m_r} = 3$. The difference between successive averages of groups is therefore three times the standard deviation of the lower group.

For each group, since the distribution of incomes is normal, we can compute the number of persons with incomes greater than any assigned limit, merely by reference to the normal probability integral table. In order to simplify our computations, we will use selected income limits. These will be m, $2m$, 2^2m, 2^3m, and so on. Let us concentrate on income $2^{s-1}m$. For the rth group the number with incomes greater than $2^{s-1}m$ is $\frac{1,000,000}{3^{r-1}} \cdot F\left(\frac{2^{s-1}m - 2^{r-1}m}{\frac{1}{3}2^{r-1}m}\right) = \frac{1,000,000}{3^{r-1}} F\left\{3(2^{s-r} - 1)\right\}$, where

$F(z)=\int_{z}^{\infty}\frac{1}{\sqrt{2\pi}}e^{-\frac{1}{2}z^2}dz$. Thus, when $r=1$, for the first group; for $s=1$, $z=0$; for $s=2$, $z=3$; for $s=3$, $z=9$; and so on. When $r=2$, for the second group; for $s=1$, $z=-1.5$; for $s=2$, $z=0$; for $s=3$, $z=3$; for $s=4$, $z=9$; and so on. We can show the appropriate values of z in the following scheme:—

Income limits	Groups 1	2	3	4	5	...
	Values of z					
m	0	−1.5	−2.25	−2.625	−2.8125	...
$2m$	3	0	−1.5	−2.25	−2.625	...
2^2m	9	3	0	−1.5	−2.25	...
2^3m		9	3	0	−1.5	...
2^4m			9	3	0	...
2^5m				9	3	...
2^6m					9	...

For our purposes, therefore, we need only look up, in the tables of the normal probability integral, a small number of values of $F(z)$. Most of these will be used several times when we form a table showing the distribution of income for the various groups. The values of $F(z)$ used are those shown below:—

Values of z	Values of $F(z)$	Values of z	Values of $F(z)$
−2.9992676	.9986469	−2.90625	.9981711
−2.9985352	.9986436	−2.8125	.9975421
−2.9970703	.9986370	−2.625	.9956675
−2.9941406	.9986234	−2.25	.9877755
−2.9882812	.9985974	−1.5	.9331928
−2.9765625	.9985425	0	.5000000
−2.953125	.9984271	3	.0013499
		9	.0000000

The number in the first group is 1,000,000. The number with incomes greater than m is 1,000,000 $F(o)$, which is 500,000. The number with incomes greater than $2m$ is 1,000,000 $F(3)$, which is 1,350. The number in the second group is 333,333. The number with incomes greater than m is 333,333 $F(-1.5)$, which is 311,064. The number with incomes greater than $2m$ is 333,333 $F(o)$, which is 166,667. The number with incomes greater than 2^2m is 333,333 $F(3)$, which is 450. $F(9)$ is so small that the frequency greater than 2^3m is zero to our degree of accuracy. Continuing in this way we get the figures in Table I.

We observe that, in this table, owing to the use of the same values of $F(z)$ in the determination of the frequencies of the various groups, the diagonal figures from income limit $2m$ onwards are successively obtained by division by 3. We have 1350—450—150—50—and so on; 166,667—55,556—18,519—6173—and so on. Naturally, therefore, the totals of the horizontal rows from income limit $2m$ onwards will be obtainable from one another by successive division by 3. We get 326,740—108,913—86,304—and so on. Of course, this is only true to the degree of approximation used in our illustration. $F(9)$ is neglected by us, but this is only because it is unimportant to our degree of approximation.

The income distribution of the whole population is given by the figures in the final column of Table I. The distribution for income limit $2m$ and upwards is of the Pareto type, where $n=Ax^{-a}$. If n_r corresponds to $x=2^r m$ and n_{r+1} corresponds to $x=2^{r+1}m$, then $\frac{n_{r+1}}{n_r}=2^{-a}$. We find, in our example, that $\frac{n_{r+1}}{n_r}=\frac{1}{3}$, therefore $a=\log 3/\log 2=1.58$. Note that we have not examined here the distribution for finer intervals between our assigned income limits. We have been content with the

TABLE I.

FREQUENCIES FOR EACH GROUP GREATER THAN ASSIGNED LIMITS.

Income limits	Groups														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	All
Number in group	1,000,000	333,333	111,111	37,037	12,346	4,115	1,372	457	152	51	17	6	2	1	1,500,000
m	500,000	311,064	109,753	36,877	12,315	4,108	1,370	457	152	51	17	6	2	1	976,171
$2m$	1,350	166,667	103,688	36,584	12,292	4,105	1,369	457	152	51	17	6	2	1	326,740
2^2m	0	450	55,556	34,563	12,195	4,097	1,368	456	152	51	17	6	2	1	108,913
2^3m		0	150	18,519	11,521	4,065	1,366	456	152	51	17	6	2	1	36,304
2^4m			0	50	6,173	3,840	1,355	455	152	51	17	6	2	1	12,101
2^5m				0	17	2,058	1,280	455	152	51	17	6	2	1	4,034
2^6m					0	6	686	427	152	51	17	6	2	1	1,345
2^7m						0	2	229	142	51	17	6	2	1	448
2^8m							0	1	76	47	17	6	2	1	149
2^9m								0	0	25	16	6	2	1	50
$2^{10}m$										0	8	5	2	1	17
$2^{11}m$											0	3	2	1	6
$2^{12}m$												0	1	1	2
$2^{13}m$													0	0	1

In this table each figure is correct to the nearest unit. Some of the horizontal totals are *apparently* incorrect.

general outline. When we go to greater detail, we find that the Pareto law does not operate.

The distribution of incomes below $2m$ is not of the Pareto type. As a matter of interest we give below the distribution for smaller intervals, showing actual frequencies between assigned limits.

Income limits	Frequency	
Below $m/2$	71,486	
$m/2 - 2m/3$	95,587	
$2m/3 - 5m/6$	155,963	
$5m/6 - m$	200,792	total 523,829
$m - 7m/6$	204,953	
$7m/6 - 4m/3$	168,254	
$4m/3 - 3m/2$	115,403	
$3m/2 - 5m/3$	72,490	
$5m/3 - 2m$	88,331	total 649,431

Comparing these figures with Table I, 523,829 = 1,500,000 minus 976,171 and 649,431 = 976,171 minus 326,740. The distribution in the lower ranges of income has a maximum somewhere near m.

The type of distribution which has been artificially constructed in the manner described in the foregoing is similar to certain income distributions, particulars of which are known. In these, there is a maximum in the frequency distribution at a comparatively low income figure, and after a certain stage the distribution is approximately of the Pareto type. Of course, we do not pretend that the origin of the known income distributions is the same as that of the distribution which has been obtained by us in our example. However, the hypotheses which were assumed are fairly plausible and it is arguable that known income distributions may be the result of the operation of causes of the kind which we have considered.

More generally, if we assume $p_r = K/b^{r-1}$, where $b > 1$, then $p_{r+1} < p_r$. Let us also assume $m_r = t^{r-1}m$ where $t > 1$, then $t_{r+1} > t_r$. The total number in the whole population is $\overset{\infty}{\underset{r=1}{S}} K/b^{r-1}$, assuming an infinite number of groups. This is $K\,b/b-1$. The total amount of income for the whole population is $\overset{\infty}{\underset{r=1}{S}} K\left(\frac{t}{b}\right)^{r-1} m = \frac{Kb}{b-t}\,m$, assuming that $t < b$ (in our case $t = 2$, $b = 3$). Thus the average income is $m\,\frac{b-1}{b-t}$ (in our case this is $2m$). If we compare the total number in the population with incomes greater than $t^s m$ with the number with incomes greater than $t^{s+1}m$ we should find, for all values of s greater than a certain value, that the ratio of these two numbers was a constant and equal to b. The Pareto law operates again, the Pareto constant α being given by $\log b/\log t$.

We may refer to the fact that the Pareto constant α is used sometimes as an indicator of the degree of inequality of incomes in a given distribution. Of two income distributions with different α's, that with the lesser α has the more unequal distribution of incomes. For our illustration the larger b is, the greater is the relative number in the first group than in the second group, and so on. For instance, if the value of b is 1,000, we may say there are 3 groups, the first with 1,000,000, the second with 1,000, the third consisting of 1 person only. There is a great deal of equality amongst the total population, 1,000,000 out of 1,001,001 being in the same group. Again, the smaller t is, the greater is α. If t is small there is not as much difference between the average incomes of the groups as for the case when t is large. Smaller values of t, associated with the same values of b, indicate greater equality in the distribution of incomes.

We have differentiated between our groups in the first place by reference to the possession by individual members of the groups of various talents which are used in the national interest; and the reward for the use of these talents constitutes the individual's share of the national income. The factor b, which has been used, indicates the manner in which the members of the population group themselves according to talents; the factor t indicates the manner in which the groups on the average are rewarded. We may say that b is a *social* factor and t is an *economic* factor. What we wish to emphasise is the obvious point that the equation

$$\alpha = \log b / \log t$$

indicates that inequality of incomes in a population is due both to social and economic factors and, in our illustration, each enters into the constitution of α in the same manner, i.e., they may be regarded as of equal importance. It is admitted that there may be ultimately a connection between these two factors, i.e., for a given population the size of b may determine the size of t, or *vice versa.* We have been concerned in this paper with demonstrating the artificial construction of a Pareto distribution, and not with the laws connecting the number of persons in different groups and their average share of the national income.

PART II

MATHEMATICAL NOTES

In the first part we assumed a number of groups with 1, 2, 3, etc., talents. We will now assume that ' talent ' is a continuous variable and that $\Pi(x)$ of the population have more than x talent, and that $-\dfrac{d\,\Pi(x)}{dx}\,dx$ of the population have talent between the limits x and $x + dx$. We will call $-\dfrac{d\,\Pi(x)}{dx} = -\Pi^1(x) = p(x)$. We will assume that the average income accruing to those with talent x is $m(x)$. We will still assume a constant coefficient of variation (λ) for all the groups. We will also assume that the type of variation is the same for all groups, but we will no longer assume normality. Let us say that the chance, in a group, of an income greater than y is $F\left(\dfrac{y-m}{\lambda m}\right)$ where m is the average income of the group and λm is the standard deviation. We shall then have the number of persons in the population with incomes greater than y in the form: $\displaystyle\int_0^\infty p(x)\,dx\,F\left(\frac{y-m(x)}{\lambda m(x)}\right)$, where we assume that x may vary from 0 to ∞, and y is constant throughout this integration. Let us call $y - m(x) = \lambda\, m(x) v$.

Thus $m(x) = \dfrac{y}{1+\lambda v}$.

Theoretically this equation will give x as a function of y and v. From this equation we get

$$m^1\,dx = -\frac{\lambda y\,dv.}{(1+\lambda v)^2}.$$

Suppose we call $p(x) = P(v, y)$, then our integral becomes $\displaystyle\int_{v_1}^{v_2} \frac{P(v, y)\,F(v)\quad \lambda y\,.}{M(v, y)\,(1+\lambda v)^2}\,dv$, where $m^1(x) = M(v, y)$. The limits of this integral are given by substitution in the equation $m(x) = \dfrac{y}{1+\lambda v}$.

$m(o) = \dfrac{y}{1+\lambda v_2}$, $m(o)$ being the lowest value of m corresponding to no talent. Call $m(o) = m_0$, then $v_2 = \dfrac{1}{\lambda}\left(\dfrac{y}{m_0} - 1\right)$. We will suppose that when $x \to \infty$, $m(x) \to \infty$ also. Then $v_1 = -\dfrac{1}{\lambda}$. Thus the number of persons with incomes greater than y in the whole population is

$$\int_{-\frac{1}{\lambda}}^{\frac{1}{\lambda}\left(\frac{y}{m_0}-1\right)} \frac{P(v, y)\,.\,F(v)\,.\,\lambda y\quad dv}{M(v, y)\,(1+\lambda v)^2}.$$

We observe that the upper limit of integration depends on the value of y.

This integral can be simplified if $P(v, y) \cdot M(v, y)$ is of the form $\phi(y).\ \Psi(v)$. In this case we get $\displaystyle\lambda y\,\phi(y) \int_{-\frac{1}{\lambda}}^{\frac{1}{\lambda}\left(\frac{y}{m_0}-1\right)} \frac{\Psi(v)\,.\,F(v)\,dv}{(1+\lambda v)^2} = \lambda y\,.\,\phi(y)\,.\,\chi(y)$. Now, for certain values of y greater than a limiting value, $\chi(y)$ may be constant to a certain degree of approximation. In that case the number of incomes greater than y is proportional to $y\phi(y)$ to that degree of approximation. It remains to find what $\phi(y)$ is. It will be appreciated that now we are dealing with a special case. We are assuming that, when the equivalent of x in terms of v and y derived from the equation $m(x) = \frac{y}{1+\lambda v}$ is substituted in $\frac{p(x)}{m^1(x)}$, the result is $\phi(y)\,.\,\Psi(v)$. Let us call $\frac{m^1}{p} = w$, where w is a function of x. Then $w\phi\Psi = 1$. If we take logs and differentiate this with respect to y we get $\frac{w^1}{w}\frac{dx}{dy} = -\frac{\phi^1}{\phi}$, the $(^1)$ meaning a first differential. But $m^1\frac{dx}{dy}(1+\lambda v) = 1$. Therefore $\frac{w^1}{w}\frac{1}{m^1(1+\lambda v)} = -\frac{\phi^1}{\phi}$ or, since $m^1 = pw$, $\frac{w^1}{w^2p}\frac{1}{1+\lambda v} = -\frac{\phi^1}{\phi}$. Differentiating with respect to v gives us $\left(\frac{w^1}{w^2p}\right)^1\frac{dx}{dv}\frac{1}{1+\lambda v} - \left(\frac{w^1}{w^2p}\right)\frac{\lambda}{(1+\lambda v)^2} = o$, since $\frac{\phi^1}{\phi}$ is a function of y only. This gives $\left(\frac{w^1}{w^2p}\right)^1\frac{m\lambda}{m^1(1+\lambda v)^2} + \left(\frac{w^1}{w^2p}\right)\frac{\lambda}{(1+\lambda v)^2} = o$, or $\left(\frac{w^1}{w^2p}\right)^1 \Big/ \left(\frac{w^1}{w^2p}\right) = -{}^{m^1}\!\big/_{m}$. In this equation there are functions of x alone. Integrating gives $\frac{w^1}{w^2p} = \frac{a}{m}$, where a is a constant. From this $\frac{w^1}{w} = \frac{am^1}{m}$, since $m^1 = pw$. Hence $w = cm^a$, where c is a constant. We also have, from $\frac{w^1}{w^2p}\,.\,\frac{1}{1+\lambda v} = -\frac{\phi^1}{\phi}$, $\frac{\phi^1}{\phi} = -\frac{a}{m(1+\lambda v)} = -\frac{a}{y}$; hence $\phi = ky^{-a}$, where k is a constant. Also, from $w\phi\Psi = 1$, we get $\Psi cm^a ky^{-a} = 1$, therefore $\Psi = \frac{1}{ck}\left(\frac{y}{m}\right)^a$, i.e. $\Psi = \frac{1}{ck}(1+\lambda v)^a$, a function of v alone.

Thus, $\chi(y) = \frac{1}{ck}\int_{-\frac{1}{\lambda}}^{\frac{1}{\lambda}\left(\frac{y}{m_0}-1\right)} F(v)\,(1+\lambda v)^{a-2}\,dv$, and, if this is for practical purposes constant for values of y greater than a certain limiting value, the number of incomes greater than y in the whole population, to our degree of accuracy, is proportional to $y\phi(y)$ which is ky^{-a+1}. In other words, the Pareto distribution emerges.

Going back to $w = cm^a$, we have, from $m^1 = pw$, $p = \frac{m^1}{c}m^{-a} = -\Pi^1$. Therefore $\Pi = \frac{1}{c(a-1)}m^{-a+1}$. We appear to have completed a circle of argument. If Π of the population have more than x talent, and those with this talent have, *on the average*, an income of amount m, and if $\Pi = \frac{1}{c(a-1)}m^{-a+1}$, i.e., if the Pareto law operates here, then, on certain assumptions, the Pareto law operates for incomes of the whole population above a certain lower limit, the Pareto constant being the same, viz., $a-1$. We only appear to have been arguing in a circle, the final statement is not the same as the original assumption. For the population arranged in groups according to talents, with average incomes appropriate, the Pareto law operates exactly from zero to infinite talent. For the population arranged according to income, the same Pareto law only operates approximately for incomes above a certain level.

As a matter of interest, if $p(x) = qe^{-qx}$ and if $m(x) = me^{ux}$, then $m^1 = mue^{ux}$ and the equation $p = \frac{m^1}{c}m^{-a}$, gives $cqe^{-qx} = mue^{ux}\,m^{-a}e^{-aux}$. Therefore $q = u(a-1)$. Thus the Pareto coefficient $a-1 = q/u$. This links up with Part I. If q is $\log b$ and u is $\log t$, $p(x)$ is proportional to b^{-x} and $m(x)$ is proportional to t^x. The Pareto coefficient is $\log b/\log t$.

We will illustrate the foregoing with an example. Let us assume that the type of distribution of income for each talent group is of Pearson Type III, this being chosen because integrals can be found in the Tables of the Incomplete Gamma Function.

If we continue to use the usual Pareto coefficient α instead of $a-1$, and say $\Pi = Am^{-\alpha}$ where A is a constant, N, the total population, is $Am_0^{-\alpha}$, where m_0 is the lowest average income corresponding to no talent. $p = A\alpha m^{-\alpha-1}\dfrac{dm}{dx}$.

The frequency greater than y is $\displaystyle\int_0^\infty p\,F\left(\frac{y-m}{\lambda m}\right)dx$

$$= \int_{m_0}^{\infty} A\alpha m^{-\alpha-1}\,F(v)\,dm, \text{ putting } y-m = \lambda m v,$$

$$= \int_{-\frac{1}{\lambda}}^{v_0} A\alpha\, y^{-\alpha-1}\,(1+\lambda v)^{\alpha+1}\,F(v)\frac{\lambda y}{(1+\lambda v)^2}dv$$

where $v_0 = \dfrac{1}{\lambda}\left(\dfrac{y}{m_0}-1\right)$.

This integral is $A\lambda\alpha\, y^{-\alpha}\displaystyle\int_{-\frac{1}{\lambda}}^{v_0} F(v)\,(1+\lambda v)^{\alpha-1}\,dv$.

Suppose $F(v) = \displaystyle\int_v^\infty f(v)\,dv$, then the integral becomes

$$Ay^{-\alpha}\left[\Big(F(v)\,(1+\lambda v)^{\alpha}\Big)_{-\frac{1}{\lambda}}^{v_0} + \int_{-\frac{1}{\lambda}}^{v_0} f(v)\,(1+\lambda v)^{\alpha}\,dv\right]$$

$$= Ay^{-\alpha}\left[F(v_0)\,(1+\lambda v_0)^{\alpha} + \int_{-\frac{1}{\lambda}}^{v_0} f(v)\,(1+\lambda v)^{\alpha}\,dv\right]$$

$$= N\,F(v_0) + Ay^{-\alpha}\int_{-\frac{1}{\lambda}}^{v_0} f(v)\,(1+\lambda v)^{\alpha}\,dv.$$

This gives the frequency greater than income y.

Let us take $\lambda = \frac{1}{4}$ (λ is the constant coefficient of variation). Let us take $f(v) = Be^{-4v}\left(1+\frac{v}{4}\right)^{15}$, where $B = \dfrac{16^{16}e^{-16}}{4\Gamma(16)}$. The β_1 of this distribution is 0.25; the distribution is moderately skew. The distribution has its mean at the origin and its standard deviation is unity. We deliberately take this value of $f(v)$ with a term $\left(1+\frac{v}{4}\right)$ in order that $(1+\lambda v)^{\alpha}$ which is $\left(1+\frac{v}{4}\right)^{\alpha}$ shall merge with this term, when we come to the integration in our expression for the frequency greater than y.

For $F(v_0) = \displaystyle\int_{v_0}^{\infty} Be^{-4v}\left(1+\frac{v}{4}\right)^{15}dv$, we have

$$1 - \int_0^{4s} \frac{e^{-t}\,t^{15}}{\Gamma(16)}dt, \text{ where } s = v^0 + 4.$$

This last integral can be obtained from the Tables of the Incomplete Gamma Function.

We assume $m_0 = £100$, and we have the following results :—

Values of			
$y(£)$	v_0	s	$F(v_0)$
0	-4	0	1.0000000
25	-3	1	.9999951
50	-2	2	.9917690
75	-1	3	.8444157
100	0	4	.4667449
125	1	5	.1565131
150	2	6	.0344001
175	3	7	.0054257
200	4	8	.0006599
225	5	9	.0000653
250	6	10	.0000055
275	7	11	.0000004
300	8	12	.0000000

If we assume the total of the population is unity, then $F(v_0)$ gives the first term in the expression for the frequency greater than v. For the second term we have to find $\int_{-4}^{v_0} Be^{-4v}\left(1+\frac{v}{4}\right)^{16.7} dv$, where we assume $\alpha = 1.7$. This value 1.7 has been obtained from actual income distributions, and it was thought worth while to introduce an element of reality into this example. This integral is $\frac{\Gamma(17.7)}{\Gamma(16)\,16^{1.7}} \int_0^{z\sqrt{17.7}} \frac{e^{-t}\,t^{16.7}\,dt}{\Gamma(17.7)}$, where $z\sqrt{17.7} = 4(v_0 + 4)$. The last integral can be obtained from the same tables as before.

We have the following results :—

Values of			
y	v_0	z	$Ay^{-\alpha}\int_{-\frac{1}{\lambda}}^{v_0} f(v)\,(1+\lambda v)^{\alpha}\,dv$
0	-4	0	0
25	-3	.9507653	.0000049
50	-2	1.9015306	.0069625
75	-1	2.8522959	.1233905
100	0	3.8030612	.3813445
125	1	4.7538265	.5157374
150	2	5.7045918	.4804890
175	3	6.6553571	.3944098
200	4	7.6061224	.3184362
225	5	8.5568877	.2611748
250	6	9.5076530	.2183983
275	7	10.4584183	.1857343
300	8	11.4091836	.1601964

From this and the previous table we find the following:—

FREQUENCIES GREATER THAN ASSIGNED LIMITS OF INCOME

Income limits (y) (£)	Frequencies
0	1.0000000
25	1.0000000
50	.9987315
75	.9678062
100	.8480894
125	.6722505
150	.5148891
175	.3998355
200	.3190961
225	.2612401
250	.2184038
275	.1857347
300	.1601964

From about £275 and onwards the factor of $Ay^{-\alpha}$ in our expression for the frequency greater than y is constant, to our degree of accuracy. This constant multiplier is 1.036952. Thus the distribution from £275 follows the Pareto law. Up to £275 the distribution may most easily be exposed by the following table, derived from the last.

Income limits (£)	Frequencies	Income limits (£)	Frequencies
25— 50	.0012685	150—175	.1160536
50— 75	.0309253	175—200	.0807394
75—100	.1197168	200—225	.0578560
100—125	.1758389	225—250	.0428363
125—150	.1573614	250—275	.0326691

There is a maximum in the neighbourhood of £120. For higher values of y the frequencies are given in the table below:—

Income limit (y) (£)	Persons with incomes greater than assigned limit
500	.06722173
1,000	.02068992
2,000	.006368069
5,000	.001341250
10,000	.0004128182
20,000	.0001270597
50,000	.00002676146
100,000	.000008236806

Thus about $\frac{1}{50}$th of the total population have incomes of £1,000 or more. On the other hand about 84 per cent. of the population have incomes of £300 or less.

We can also compute the distribution of aggregate income. From the fact that $A\alpha\, m^{-\alpha-1}\, dm$ persons have an average income of m, we find the total income of all is $\int_{m_0}^{\infty} A\alpha m^{-\alpha}\, dm$ which is $\frac{A\alpha}{\alpha-1} m_0^{-\alpha+1} = \frac{N\alpha}{\alpha-1} m_0$. With $N = 1$, $\alpha = 1 \cdot 7$, $m_0 = 100$, this is 242.8571. The distribution of amount of income is given by the table below:—

Income limits (£)	Amount possessed by persons with incomes greater than assigned limits (£)
25	242.8571
50	242.7997
75	240.7319
100	230.0603
125	210.2525
150	188.6990
175	170.0874
200	155.0071
225	142.7507
250	132.6024
275	124.0443
300	116.7145
500	81.62642
1,000	50.24695
2,000	30.93062
5,000	16.28661
10,000	10.02558
20,000	6.171470
50,000	3.249606
100,000	2.000367

Thus about one-fifth of the total income accrues to those with more than £1,000 income, about $\frac{1}{50}$th of the population. About one-half of the total income goes to the 84 per cent with less than £300 income.

The full account of the distribution in the lower ranges is given by the following table:—

Income limits £	Frequency	Amount of Income (£)	Average Income (£)	Mid-range (£)
25— 50	.0012685	.0574	45.3	37.5
50— 75	.0309253	.0678	66.9	62.5
75—100	.1197168	10.6716	89.0	87.5
100—125	.1758389	19.8078	112.6	112.5
125—150	.1573614	21.5535	137.0	137.5
150—175	.1150536	18.6116	161.8	162.5
175—200	.0807394	15.0803	186.8	187.5
200—225	.0578560	12.2564	211.8	212.5
225—250	.0428363	10.1483	236.9	237.5
250—275	.0326691	8.5581	262.0	262.5

From this table we get confirmation of the existence of a mode in the neighbourhood of £120; it is near £112.5.

We may utilise our theoretical distribution of incomes to give some general results which are like certain facts relating to income in the U.K. If the total population is 20,000,000, the aggregate income is £4857 mn. 3,038,000 persons have less than £100 income. 10,580,000 persons have incomes between £100 and £200. 3,178,000 persons have incomes between £200 and £300. The total income of all those with less than £300 income is £2,523 mn. The number of surtax payers, with incomes of £2,000 or more is 127,361 and their aggregate income is £618,612,000. There are 535 persons in the millionaire class with incomes of £50,000. Their aggregate income is about £65 mn., about $\frac{1}{80}$th of the total income of the whole population.

We have been able to show that, approximately for higher incomes, the Pareto distribution can be derived from comparatively simple hypotheses. For the lower income ranges the distribution has a mode not located at the lowest income. Naturally we are not able to deduce that a given income distribution comes from the same simple origins. Our hypotheses include the constancy of the coefficient of variation and the constancy of the type of distribution of income of those in the same talent group. These hypotheses are plausible; for certain occupational groups we have seen statistical data where the coefficient of variation is fairly constant, and where the type of distribution seems to have similarities from one group to another. We found that, at a certain stage in our argument, we were virtually assuming that the connection between numbers in talent groups and their average income was given by the Pareto law. We have not demonstrated that this law can be derived exactly from the operation of simple economic factors; in our exposé, it emerged as a result of a certain mathematical assumption, for which we had no justification apart from expediency.

Commodity Stocks and the Trade Cycle

By K. Lacey

MR. T. BARNA's article, "Valuation of Stocks and the National Income", in ECONOMICA of November, 1942, must have provided valuable material and argument for the correction of statistics of National Income and Investment, particularly in regard to the effect of changing price levels upon those statistics. In the subsequent White Paper calculations of National Income, Mr. Barna's suggestion of eliminating movements in the book values of replaced commodity stocks from the computation of National Income has not been accepted, but it has been recognised that such movements must be included in the value placed upon ' investment ' in stocks.

In this respect the ' official ' point of view would appear to be in accordance with the facts of the situation. Under our present Accounting System (the writer is an accountant) a replacement of commodity stocks at a higher cost price does, to the extent of the increase, appear in the accounts as additional ' investment ', and a replacement at a lower cost price is reflected by a reduction of the booked value of ' investment '. In most cases stocks are valued on a first-in–first-out basis, so that stocks on hand are regarded as composed of the latest acquisitions. The consequence is that stock values, in so far as they comprise materials or finished products acquired at market prices from other traders, increase considerably during a boom, and fall drastically after the crisis of the Trade Cycle.

The movement in stock values does not, however, keep a constant pace with the movement in market values. Stocks are valued at cost, which means the purchase cost at market prices of raw materials or finished products bought from other traders, plus the factor cost of work done upon them. If, on average, stocks equivalent to three months' turnover are carried, then book cost, in respect of the part represented by outside acquisitions, will be the average of the previous three months' market prices. But after the crisis, book cost will almost inevitably be in excess of the current replacement cost of the stocks (or current selling price less selling expenses in the case of finished products) and in accordance with the present accounting practice such stocks would be written down to market value. It is only at this point that book value and market value coincide, and even then there are likely to be some items of stock composed mainly of factor cost, the book value of which will be below market value. The element of factor cost in stock values will, of course, tend to rise and fall during the upward and downward movement of the Trade Cycle, but in practice the movement is not nearly so violent as the fluctuation in book values due to changes in the selling prices of raw materials or finished products passing from one trader to another. For the sake of completeness and to avoid misunderstanding, it should perhaps be explained that in computing profits, both for accounts and for income tax purposes, the above method necessarily involves continuity in stock valuation as between one period and another. That is to say, if stocks at the end of period 1 are valued at cost, then opening stocks in period 2 must also be valued at cost even though closing stocks for period 2, owing to a fall in market values, are valued at market prices. Similarly in period 3, opening stocks must be valued at current market values even though ending stocks, owing to a rise in prices, are valued at cost.

The above is rather an oversimplified description of a complex accounting system, and it would need a long article by an abler pen than mine to give a really adequate exposition of existing accounting methods, including variations of practice between classes of traders and between different professional ' schools '. Nevertheless it will serve as an introduction to a more important matter, namely, the connection between stock valuation and the Trade Cycle.

It must now be recognised that income is not computed on the basis of ' maintaining the real amount of capital assets intact ', but merely on the basis of maintaining intact the financial value of the capital, including business capital held in cash form and deducting bank overdrafts and other debts. Owing to the use of the first-in–first-out method of valuing stocks, the effect is that the real value of business capital becomes 'watered down' as prices rise, with a consequent inflationary effect upon profits, and that such 'water' is squeezed out when prices fall, with a consequent deflationary effect upon profits. The balancing factor in this contraction and expansion of the real business capital is normally in the financial position of the business. When capital is 'watered down', it does not necessarily involve a reduction in the quantity of fixed and floating assets carried (often the reverse), but rather a reduction in the cash balance of the Firm or an increase of indebtedness to Banks or others. And an increase in the real worth of the business when prices fall usually means an improvement in the cash position or a reduction of indebtedness.

In other words, as a result of self-deception, traders are pumping money into circulation as income when prices are rising, and are therefore producing an inflationary movement which is certain to end in a collapse. When the collapse does arrive, traders then begin to withdraw money from circulation and thus prolong the depression.

The above argument is only completely true to the extent that rises or falls in the book values of replaced stocks are reflected in increased or decreased income payments to proprietors and shareholders. In practice there is a tendency to retain more profits in the business when prices and profits rise, and to reduce the retained amount when prices and profits fall. But this stabilising tendency does not operate to anything approaching the required degree, and there is little doubt, as far as the short-term fluctuations of the Trade Cycle are concerned, that both the upward and the downward movements are due to self-deception. As soon as market prices begin to rise at the end of a depression, traders buy in larger quantities of their requirements of raw materials, etc., and the result is an increase in prices and an inflation of profits at the expense of capital in cash form. This brings in its train an increase in real investment and income, but the movement is unstable and collapses as soon as the artificial movement of prices collapses. But the fall in price results in self-deception in the opposite direction, which in time wipes out the unstable progress during the boom, meanwhile creating unemployment owing to surplus stocks and surplus productive capacity relative to the reduced aggregate income. Incidentally, the downward movement in aggregate income is more rapid than the upward movement because of the change in the basis of stock valuation, which creates (before replacement) an immediate financial disinvestment in stocks, as compared with the more gradual increase of investment in stocks on the upward movement as and when such stocks are actually replaced.

The figures given by Mr. Barna of investment and disinvestment in stocks, including movements in the replacement cost of stocks, appear to support the above argument. The depression of 1930–34 was unduly prolonged, and the subsequent upward movement did not absorb all the unemployed or raise prices back to the 1929 level, but this was due to the secular deflationary tendency with which this article is only incidentally concerned. It is merely maintained that the oscillation itself, and not necessarily the long-term average level of employment or prices, is due to the self-deception described above.

We can see how this instability operates by considering it from the angle of the investment multiplier. It seems reasonable to assume—and this is borne out by Mr. Barna's figures—that new investment in increased quantities of stocks or minus investment in stock quantities are mainly due to the movements of price, more stocks being acquired in times of rising prices and less when prices are falling. On broad lines, therefore, we can take the whole of the variation in investment in stocks, both in prices and quantities, as due to the self-deception induced by our present accounting system. If we consider Mr. Barna's figures of 'apparent', i.e., financial, investment in stocks for the years 1928–38, it seems obvious that the disinvestment in stocks during the years 1929–32 had a multiplied effect upon aggregate income much greater than the multiplied effect of the fall in investment in fixed capital. And the investment in stocks during the years 1933–37, including both quantity and price, was at least as important in raising monetary income as the additional investment in fixed capital during those years. The fall in prices during the autumn of 1937 produced a further depression in U.S.A. in 1938 and onwards, and would have had the same effect in U.K. (it did for a while create an increase in unemployment) but for the intensified armament expenditure.

It is unsatisfactory perhaps to include increases or decreases in book values of replaced stocks in 'investment' when in reality they merely involve financial self-deception. The position can, however, be expressed more realistically by limiting 'investment' to increases in quantities of fixed and floating capital but by regarding the writing-up of replaced stocks, in so far as it leads to a distribution of profits in excess of 'real' profits, as dissaving. In the same way the writing down of stocks leading to an under-distribution of real profits would be regarded as additional saving. On this basis the Trade Cycle fluctuation is due to dissaving by traders during the boom and additional saving during the slump. And it needs to be remembered that this dissaving and saving has its multiplied effect upon aggregate monetary income in the same way as fluctuations of real investment.

The above method of expression may assist us better in considering how we can bring Trade Cycle fluctuations under control. The remedy would appear to lie in an alteration of our accounting system with a view to avoiding this alternation of dissaving and saving. To private traders owning their own businesses, accounting systems are not perhaps important. If, for example, a private trader makes large profits owing to rising prices, but finds his business cash disappearing, he is likely to leave a large part of his increased profits in the business; and if profits are diminishing owing to falling prices, but his cash is mounting, he may not be averse to withdrawing some of that cash to meet his personal expenditure. This is not possible under our present system of Joint-Stock enterprise and large Trusts. A shareholder cannot expect a favourable reply if he writes to a Company asking for a return of some of his capital to supplement his smaller dividend, and herein may lie the explanation of the increasing severity of cyclical oscillation *pari passu* with the increase of 'Big Business'. There is the further peculiar effect that during a period when capital is being 'watered down', the exchange value of that capital, i.e., the selling value of the shares representing that capital, being based upon the fictitious income yield, shows a very considerable increase, whereas there is the opposite effect when the real value of the capital is being increased. Our hypothetical shareholder is not likely to decide to obtain the additional cash he needs by selling part of his holding on the market at the ridiculously low levels prevailing during the slump.

When we think on the above lines, the economist's conception of keeping intact the real capital is far more sensible than that of the business man and accountant. It is an absurd position when an increase in prices on sales between traders can create a fictitious period of prosperity, and when a fall in prices in respect of such sales can throw us into a welter of distress, poverty and unemployment. But if the remedy is to compute our incomes on the basis of keeping intact our real capital, what does this involve in accounting procedure? In brief, the answer is that we must value our stocks on the last-in–first-out method instead of the first-in–first-out method as at present. To put it in more homely terms, the cost of a sale should not be regarded as the cost of the actual article sold but as the cost of replacing that article, because only by replacing it does the trader keep his real capital intact. In practice, as already suggested, the nearest we can reach to this ideal position is to treat the sale as satisfied out of the latest additions to the stock in question. On this basis new stocks acquired or produced are constantly being sold (in theory), leaving a 'base' stock at a constant book value. This base stock will, of course, vary in quantity, being added to as business expands, and the book value will increase as the quantity of base stock increases. But there will be no variation in value in respect of increases or decreases in the cost of replacing the base stock, because in theory the base stock is always the same stock, or at least is regarded as a fixed investment in stock which must be given the same value even when replaced at a higher or lower price.

The last-in–first-out method of valuing commodity stocks is no mere theory. It is already adopted in a few instances for certain types of stocks in this country, and is having an increasing vogue in recent years in U.S.A.—to such an extent in fact that the U.S. Tax Law now gives the taxpayer the option of adopting this method for purposes of his tax returns. The last-in–first-out method (Lifo as it is called in U.S.A.) is mainly adopted by U.S. Corporations, the selling prices of whose products are fairly closely tied to market prices of raw materials. The effect of the new method is to level out their profits by lowering the peaks and raising the valleys. There is no doubt that an extension of this method would have a stabilising effect upon our economic system. There are many other types of business (e.g., those producing proprietary lines), the selling prices of whose products lag very far behind the movement in raw material prices, and tend rather to be based upon the average cost of their stocks on hand. The profits of such businesses on the first-in–first-out basis do not vary quite so greatly over the Trade Cycle, and the adoption of the last-in–first-out basis might have the unusual effect in some instances of making their profits more unstable from year to year than they are at present. There is a fallacy here, however, and it must not be assumed that the mere earning of a reasonably stable profit is evidence that no self-deception exists and that no alteration of method is desirable. The position here is that sales are made at too low a price relative to replacement costs when market values are rising, and at too high a price relative to replacement costs when market values are falling. Such businesses are not essentially different from other businesses, and like them go into debt during the upward movement of the Trade Cycle, and accumulate cash during the depression. The remedy in their case is the adoption of the last-in–first-out method coupled with an alteration of policy whereby selling prices are based on current costs instead of on stock cover costs. Paradoxically such a policy would not have the result of raising selling prices still higher during a boom, despite the fact that such prices would be based on replacement costs. On the last-in–first-out method, any tendency by manufacturers to anticipate their raw material requirements would not have nearly so great an inflationary effect upon aggregate income and future prices as at present, because it would not involve an increase in the value of base stocks of raw materials. The present cumulative features whereby prices react on base stock investment, base stock investment on aggregate income, and aggregate income upon prices, would largely disappear.

There are so many aspects of this question that it is difficult to deal with it adequately in an article. But one alternative method of computing income on the basis of keeping intact the real capital must be mentioned, namely the method adopted in France in 1939. Traders, whilst retaining the first-in–first-out method, were permitted to create a reserve out of profits equal to the additional cost of replacing their stocks. This reserve was free of income tax and war tax so long as it was retained in the business. If and when prices fell later the reserve would be correspondingly reduced, and the part of the reserve so released would become liable to taxation. This method has almost the same effect as the last-in–first-out method, but it throws into greater clarity the position of British traders paying E.P.T. at 100 per cent. No such reserves are allowed for tax purposes in U.K. and consequently when stocks are replaced in full after the War, British traders will be in debt to the extent of the increased cost of replacement, except in so far as they have retained profits in the business out of their wartime earnings less income tax and E.P.T. Unless some constructive ideas arise out of the consultations initiated by the Chancellor of the Exchequer on the subject of taxation rebates on profits retained in the business, it is likely that in the post-war years some French and American businesses will be in a stronger financial position than similar British businesses. Having in mind our Trade Cycle analysis, however, it is to be hoped that these consultations will not be concerned purely with the expected inflation in the immediate post-war years (this would give the trader all the advantages with none of the disadvantages), but will also take into account the deflationary effect of our accounting system when prices fall.

If agreement should be reached as to the advisibility on economic grounds of adopting the last-in–first-out method after the war, both for accounts and for tax purposes, there would be a strong case for granting some taxation relief in respect of the wartime overtaxation, i.e., overtaxation on the basis of keeping the real capital intact. In fact, unless some relief were granted, the last-in–first-out method might not be sufficient to save us from a depression if prices fell later. The adoption of the new method in the immediate post-war years would mean that base stocks would appear in the books at a high value, and when prices fell, auditors would probably require traders to write down such stocks to market value, if indeed the traders themselves had not already done so. The cumulative deflationary effect of such a proceeding has already been described. A tax credit equal to tax on the wartime rise in replacement costs of pre-war stock quantities might be regarded as too generous vis-á-vis other taxpayers, who also suffer through wartime inflation, although it is what the French and U.S. provisions involve. But at least an income tax post-war credit could be granted as an equivalent to the post-war credit of individuals. This would be additional to the E.P.T. post-war credit, which does not benefit all traders. Both the income tax and the E.P.T. post-war credits would be allowed to traders only on condition that they were put to a stock reserve. This reserve would then be regarded as a cushion to cover the possibility of replacement costs falling below book costs on the last-in–first-out method, the object being to avoid the necessity of current profits being drawn upon to meet a temporary decline in the replacement cost of stocks. A more difficult question is how to provide a tax free cushion reserve to new entrants to industry and trade, but it is not beyond the wit of lawyers and accountants to devise some reasonable scheme. Another point to be remembered is that if traders adopt the last-in-first-out method in the immediate post-war years they will not obtain the benefit they would otherwise have done under our present accounting system on the occasion of a subsequent deflation of prices (cf. the E.P.D. repayment claims in 1920 owing to falling stock values). This is an argument for adequate tax credits in respect of wartime inflation.

The above paragraph should not be regarded as putting forward specific proposals, but rather as showing that it is practicable so to amend our accounting and taxation system that income shall be computed on the basis of keeping intact the real capital. But the change cannot be justified purely on accountancy grounds—accountancy, like finance, must be our servant and not our master—and before any change can be made, the necessity of the change must be accepted on scientific grounds by economists and others.

The question may be raised as to why we need to concern ourselves with short-term fluctuations of trade. Why not concentrate on the long-term issue raised by Lord Keynes in his *General Theory* and see that ‘ real ’ investment and the size of the multiplier are such that full employment is assured ? The last war, and the present war, have shown that it can be done. The answer is that we can afford to ignore the possibility of short-term fluctuations when consumption goods are relatively scarce, such as in wartime, but that we cannot solve the problems of plenty without considering both long- and short-term fluctuations. Our present accounting system is in the ‘ wrong gear ’—it is geared for scarcity and not for plenty. If, for example, the harvest is relatively poor or supplies of minerals are diminishing, the effects are, first that prices rise, secondly that base stock values are increased as stocks are used up and replaced, thirdly that there is the multiplied effect upon aggregate income of the rise in stock values, which leads, fourthly, to a further rise in prices. The result of these influences is that an over-stimulating demand is exerted upon primary producers, but as soon as the apparent shortage is cured, prices fall, and the flaw in the ‘ gearing ’ of the system becomes obvious. Base stocks are written down to replacement cost or selling price, with multiplied effect upon aggregate income, the consequence being that consumer demand is reduced and raw material prices fall even more heavily.

The effect upon primary producers of the over-stimulating boom demand is shown clearly in the article “ Commodity Stocks in the Trade Cycle ”, by L. M. Lachmann and F. Snapper (ECONOMICA, November, 1938). The increased consumption brought about by the cumulative price inflation, added to the speculative acquisition of stocks by manufacturers, etc., appears to have the frequent result of leading to an apparent shortage of visible raw material stocks during the boom and a glut during the slump (see pp. 436–7). The reduced visible stocks during the boom are due to a demand which cannot last indefinitely because it is induced by inflation and speculation, but since production of primary products is rather inelastic, the increased output created by the boom cannot quickly be curtailed. Consequently, when consumer and speculative demand falls with and after the crisis, visible stocks jump up immediately. Manufacturers, etc., not only need less raw materials to satisfy immediate consumption but also allow their stock cover to run down, thus forcing the ‘ surplus ’ stocks upon dealers and producers. It is difficult to see how the end of prosperity can be said to be due to “ the scarcity of certain factors of production ” (page 445) since the crisis is always associated with a fall in prices. It is surely more likely to be due to a surfeit of stocks in the hands of manufacturers, etc., and statistics given by Messrs. Lachmann and Snapper indicate that visible raw material stocks are beginning to mount before the crisis (see, for example, the increases in visible stocks in 1937, page 437), which shows that manufacturers’ acquisitions are dropping below the output of raw materials. Similarly there is evidence that visible stocks begin to fall before there is any noticeable recovery. Moreover, a comparison of the statistics of Messrs. Lachmann and Snapper with those of Mr. Barna would appear to indicate that visible raw material stocks move in inverse ratio to total stocks and that, despite the increased consumption during the boom, total stock quantities are not only maintained but increased, and similarly that, despite the accumulation of visible stocks during the slump, some disinvestment in total quantities does take place.

The position, therefore, appears to be that the present accounting system, coupled with the tendency by manufacturers, etc., to anticipate their stock requirements during the boom, has the result of accelerating consumer demand and of accelerating the forward movement of stocks along the productive line even more, so that stocks accumulate at the consumer end of the line where their value is highest (more factor cost and entrepreneurial profit included in their book value). After the crisis the reverse tendency operates; consumer demand slows down and the forward movement in stocks slows down even more, with stocks accumulating at the producer end of the line where their value is lowest. Messrs. Lachmann and Snapper point out (page 439) that the market price of raw materials moves in inverse ratio to the quantity of visible stocks, so that even if there were no ' real ' investment in additional stock quantities during the boom, there would still be new ' financial ' investment as raw materials moved more quickly forward and were booked by manufacturers, etc., at the higher market prices. Similarly there would be ' financial ' disinvestment after the crisis as manufacturers stocks were written down to market values, and as the new output of raw materials accumulated with producers and dealers at factor cost or at low market values.

It would appear, therefore, that no long-term remedies for our economic ills are likely to succeed unless we change the gearing of our accounting system. If, for example, it is desired to stimulate investment and consumption with the object of attaining full employment, the position is likely to be satisfactory so long as prices are rising, but as soon as prices fall there is a drop in aggregate income of such a magnitude that efforts to maintain it are not likely to succeed, and the original plan is likely to become suspect. This was the experience of the New Deal—it was a galloping upward movement in both prices and quantities—and the experiment became somewhat discredited because it produced a slump as soon as supplies became adequate. It is this drop in the book value of stocks which makes our existing depressions so intractable, and as already stated, we need to ' re-gear ' our system accordingly in order to make it possible for us to succeed in our long-term plans.

The last-in–first-out method would appear to be the right ' gear ' for a potential age of plenty. Instead of income rising when supplies are short and falling when supplies are realised to be large—which is what happens at present—incomes would tend to rise with an increased quantity of commodities and to fall with a reduced quantity. We should then be organised for plenty instead of for scarcity. This does not mean that in normal times a stimulation of consumption and investment is unnecessary. There is no doubt that the depression of 1930–34 was intensified owing to the secular tendency to oversaving. The last-in–first-out method would assist in ironing out the boom and the slump, but would not prevent a steady fall in prices if there were aggregate oversaving. And we cannot have a sound economy with falling prices, unless—as is unlikely—it is possible for the trader to share the burden of the fall easily with the other factors of production. We need, therefore, the combination of a change in the gearing of our system coupled with a plan (after war shortages are cured) for reducing savings and increasing consumption and investment.

The benefits of the change in the gearing of the system can be summarised as follows:—

(1) Since people often save more than they would otherwise do because of the risks of the present instability, the ironing out of boom and slump should be beneficial in reducing savings.

(2) The harvest is unpredictable and we cannot adequately plan agricultural output. The new gearing should prevent aggregate income from falling merely because of an abundant harvest leading to lower prices, and should enable us to enjoy our abundance.

(3) A planned increase of consumption and investment will probably involve some rise in prices, particularly if unemployment exists at the time the scheme is commenced. The new gearing should prevent this rise from having a cumulative inflationary effect upon income with the likelihood of a later relapse. The cumulative effect will be limited to price rises on new investment in stocks and fixed capital and will not be extended to price rises on replacements of existing stocks.

(4) The new gearing should act as a buffer against price movements originating in other countries continuing on the old basis. It is better, of course, if all countries can be brought into line in this respect.

Finally, we need to consider the position regarding replacements of fixed assets. It would be difficult to adopt a last-in–first-out method for replacements of buildings, machinery, plant, etc., because there is such a long time-lag between acquisition and replacement, and because of the physical difficulty of earmarking certain outlays as being replacements as distinct from additions. For those who are interested, however, a scheme for avoiding fluctuations in the book value of fixed assets as they are replaced was put into operation by the French Fiscal Authorities in 1939, and although complicated, it is not unworkable. Nevertheless the matter is not nearly so important as in the case of commodity stocks. Whereas in pre-war years commodity stocks in

U.K. of say mill. £1,600 were replaced completely in six months, replacements of fixed assets in the same period were in the region of only mill. £200. Moreover, fixed assets themselves are commodity stocks as far as their producers are concerned, and if we can smooth out price movements on stocks and maintain a stable level of aggregate income, the price of fixed assets should also be reasonably stable.

If, however, owing to instability in other countries, price movements remain violent, the position of the community could be safeguarded by restricting new additions to fixed assets when replacements are costing more and by stimulating new additions when the cost of replacements is low. In fact this should tend to operate automatically, because the value of new additions to fixed assets is computed by deducting from total expenditure on additions and replacements the sum set aside for depreciation, with the result that any difference between the actual cost of replacement and depreciation (the latter representing what would be the average annual expenditure on replacements if they were evenly spread and if the price level had not altered since the first acquisition) is already included (plus or minus) in additions and would be the subject of any social control which came into operation.

Enough has been written to give an outline of the relation of commodity stock valuation to the Trade Cycle. The ideas put forward need further study, particularly by economists, from whom any suggestion for changing our present accounting system must come. It is in the hope of making a *prima facie* case for an urgent study of the problem that this article is written.

Public Expenditure in the National Income: A Note

By K. W. Rothschild

THE regular publication of a White Paper on National Income as well as the rich crop of publications on this subject in the United States have led to a general increase in interest among economists and administrators in questions relating to the methods of estimating and interpreting National Income figures. While definitions, procedures, etc., are still in a fluid and formative state, it is important to avoid any unnecessary ambiguities in the basic problems. It is, therefore, I hope justifiable to point out some misunderstandings and ambiguities which have crept into an article by Ernest H. Stern, bearing the same title as this one.[1]

I

The mistake in Mr. Stern's paper seems to me to lie in his attempt to link together two problems—viz., the treatment of indirect taxation and the classification of government expenditure—although they are really of quite a different nature.

The major part of Mr. Stern's paper is taken up with a very real problem of measuring National Incomes. He tries to find a practical way of dividing Government expenditure[2] into two categories: expenditure which only aids production in the private sector of industry, and expenditure which provides for the collective needs of citizens or leads to an increase in the assets of the nation. The existence of this problem has, of course, been known before. The inclusion of the first type of expenditure in the National Income is a source of double-counting, since the value of these goods and services is once counted when purchased by the Government, and is then again included in the value of the final goods to whose output this public expenditure has contributed. Economic statisticians have been aware of this source of error. But faced with the extremely difficult task of dividing Government expenditure into the two above-mentioned categories, they have decided to regard *all* public expenditure on goods and services as falling into the second category, and have therefore included it in the National Income. Mr. Stern now proposes a solution to this problem. I shall make a few remarks on these proposals in the second section of this article. But first I should like to clear up the major misunderstanding which occurs in the short first section of Mr. Stern's article.

In this section, the author deals with the method adopted in the White Paper on National Income (Cmd. 6347/42) and the article by J. E. Meade and Richard Stone "The Construction of Tables of National Income, Expenditure, Savings and Investment"[3], which gives a short description of the methods followed in the White Paper. Mr. Stern first states, quite rightly, that the authors of the White Paper include all payments received from public authorities in the National Income, i.e., they treat all the public expenditure as an *addition* to the output of the private sector. In this they followed the accepted practice of most writers in this field of economics. And Mr. Stern does them probably considerable injustice when he says that "they do not contemplate the possibility that part of the public expenditure may be made for the purpose of maintaining the earning power of the national community"[4]. They, like others, were probably quite aware of this possibility. But, again like other statisticians, they came to the conclusion that in practice there is no way in which it can be decided what part of the Government's expenditure is spent in assisting the productive process, and they therefore treated the whole of the public services as direct services.[1] Nevertheless, here certainly exists a debatable point, and so far so good.

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[1] ECONOMICA, May, 1943, pp. 166–75.

[2] Excluding transfer payments.

[3] *Economic Journal*, June-September, 1941.

[4] Stern, op. cit. p 167

But in the next paragraph Mr. Stern accuses the authors of the White Paper of inconsistency in applying their method of including all public expenditure in the National Income. " The authors of the White Paper must have felt ", he writes, " that income derived from public expenditure, which itself is derived from taxes, may be counted twice, if taxes were not deducted from the income. At any rate, they did deduct taxes; however, not all of them, but only those of an indirect nature ".[2] And he comes to the conclusion that " the mistake of the authors of the White Paper and Meade–Stone lies in their choosing—for the elimination of double-counting—the mode of taxation instead of the services performed by the recipients of the taxes ".[3]

In other words, Mr. Stern assumes that the deduction of the indirect taxes has the function of avoiding the double-counting of a certain proportion of public *expenditure* (which may or may not correspond to the actual proportion spent in assisting the productive process and which should be deducted on theoretical grounds). This is, however, not the case. The special treatment of indirect taxes has nothing whatsoever to do with the division of public expenditure into the two categories we mentioned before. The White Paper is quite consistent in including *all* goods and services purchased by the Government in the National Income. The deduction of indirect taxes follows an entirely different purpose. It serves to prevent National Income estimates from being influenced by mere changes in the mode of taxation.

For the purpose of this article, we may explain the matter best in the following way.[4] The Government can raise its ordinary revenue either by direct or by indirect taxation. We must see to it that in calculating the National Income the mode of taxation does not influence our result, if the real output of goods and services is the same in both cases. Now, in this connection we can say that in essence the difference between a direct tax and an indirect tax is that the former is paid directly by the citizen to the tax-collector at certain times of the year, while the latter is paid on certain occasions—the purchase of an article, for instance—to an intermediary (the seller of the article, in our case) who later hands on the sum to the tax collector. The direct taxes *received* by the tax collector, i.e., by the State, are, of course, not included in the National Income. Tax revenue represents a mere transfer and must therefore not be included in the National Income. But if we calculate the National Income at " market prices ", then we include in the National Income the indirect taxes when they are *received* by the State (through sellers, brokers, etc., who act as the Government's tax collectors). The deduction of indirect taxation from the National Income at " market prices " serves to exclude transfer payments to the State from the National Income estimate. It puts indirect taxes on the same level as direct taxes and so prevents the National Income estimates from being disturbed by the mode in which the Government collects its taxes. But as far as public *expenditure* is concerned no difference at all is made between the sources of the Government's revenue; *all* expenditure on goods and services is included in the National Income.[5]

Parallel to this misapprehension with regard to the treatment of indirect taxation in the White Paper runs a curious interpretation of the meaning of " factor cost ". The White Paper and Meade–Stone denote the National Income in which goods and services are valued net of indirect taxes as " National Income at factor cost ", for the simple reason that it is equal to the incomes which all factors of production receive *qua* factors of production and not *qua* tax collectors of the State. Mr. Stern attacks the term " factor cost " with the following argument: " The income received by the factors of production is always recorded at ' market prices ' and their sum total can be no other than total income at market prices. The deduction of indirect taxes would make it into a total ' at factor cost ' only if the indirect taxation were equal to the cost of distribution, which it is obviously not, and is not claimed to be by Meade–Stone ".[1] If I understand this passage rightly, it means that Mr. Stern believes that National Income " at factor cost " should represent the aggregate of all incomes generated through the output of goods and services *at the place of production* (i.e., National Income at wholesale prices), while National Income " at market prices " would also include incomes generated in distributing those goods and services (i.e., National Income at retail prices). Now if this interpretation is correct, then Mr, Stern uses the terms " factor cost " and " market prices " in a way which is totally different from the generally accepted usage. For, normally, no difference is made between factors of production employed in ." production " and those employed in " distribution ", a distinction which bristles with doubtful border cases. All that " National Income at factor cost " really represents is the aggregate income of *all* factors of production. This will, however, fall short of the " National Income at market prices " by just that amount of money which, though spent in the market on goods and services, does not become income of any factor of production. And this difference is obviously equal to indirect taxes[2], which are paid by purchasers, but represent a transfer to the Government, not an income of a factor of production.

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[1] That this is the only way out in practice is, for instance, held by J. R. Hicks. See *The Social Framework*, p. 135.

[2] Op. cit., p. 167.

[3] *Ibid.*, p. 168.

[4] For different approaches see, for instance, Meade–Stone, op. cit., pp. 217–8, and Hicks, op. cit., Appendix F.

[5] This fact seems to have been overlooked by Mr. Stern and is probably the reason for his misunderstanding. He writes: " They (Meade and Stone) apparently, though tacitly, assume that the money collected by indirect taxation is taken out of the circulation. Nothing of this kind happens. The facts are: (1) The money raised by indirect taxation reverts to the economic community in exactly the same manner as money raised by direct taxation and is spent on services rendered and, therefore, included in the income of the factors of production in exactly the same way as the money raised by direct taxation ". (Op. cit., p. 167.)

II

It has been mentioned before that the major part of Mr. Stern's paper is taken up with an attempt to divide public expenditure into a " costs of production " category which only aids the output of goods and services in the private sector and should therefore not be included in the Net National Income, and into a " comfort " and " savings " category which provides additional goods and services for the public and is therefore part of the Net National Income. Here Mr. Stern deals with an important and intricate problem, a solution of which would certainly give greater significance to National Income figures.

When it comes, however, to the actual solution introduced by Mr. Stern one cannot help feeling that his proposal suffers from the same inherent difficulties as the method adopted in the White Paper and attacked in the first part of his article. Mr. Stern divides public expenditure (apart from transfer payments) into " security expenditure ", which comprises expenditure of the Home Office, the Armed Forces, the Foreign Office, the corresponding departments of Local Authorities and any other expenditure for the maintenance of security, law and order, into " comfort expenditure " comprising expenditure for education, health, improved housing, recreation and the amenities of life, and " savings " representing the net increase in tangible and perhaps intangible assets. Then, while the existence of border cases is admitted, " comfort " expenditure and " savings " are included in the Net National Income, whereas it is rather dogmatically stated that " security " expenditures are in the nature of " costs of production " and form, therefore, no part of the Net National Income. This is so, because " they are prerequisites of, and instrumental to, attaining a certain level of production or safeguarding the earning power of each productive unit of the nation ".[3]

Now while this is certainly true for a considerable proportion of a Government's security expenditure, it is hardly sufficient to characterise the whole of it. We can, for instance, imagine a community of rentiers who do not produce anything and who will nevertheless find it desirable to provide collectively law courts, defence services against aggression from outside, etc. Here we see clearly that security expenditure also represents the satisfaction of collective wants and is, to this extent, an addition to the output of the private sector. The fact is that security expenditure contains elements of a " cost of production " nature and elements of a " comfort " nature, and it is impossible to separate them. One may adopt Mr. Stern's solution and regard the whole as falling into the first category, or one may follow the practice of the White Paper and other authors and include the whole of it in the Net National Income. Which solution one chooses will be partly a matter of judgment, but it will also depend on the purpose for which the estimates are needed, and on the methods adopted by other writers with whose results the figures are to be compared. But in any case one will have to keep in mind the existence and the nature of the—so far—insoluble difficulties involved in the item " public expenditure ", and the time has probably not yet come which would allow any dogmatic statement in favour of any particular method.

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[1] Op. cit., p. 168.

[2] The argument can be easily extended to cover the case of subsidies.

[3] Op. cit., p. 170.

In the third section of his paper, Mr. Stern makes a new suggestion for the calculation of National Income figures. Because he regards security expenditure as a " cost of production " which aids output in the private sector of industry, he realises that certain types of non-recurring security expenditure represent an asset that will aid production over a period of years; and its cost should therefore be spread over that whole period. War expenditure is given as an example, the cost of war being part of the costs of production not only for the war period but also for the ensuing peace period. The total war expenditure should, therefore, be allocated to a period that covers the war and the probable length of the following peaceful inter-war period. With the next war a new cycle begins.

It will be clear that this problem does not arise if the security expenditure is not regarded as " costs of production " but as the provision of goods and services for the satisfaction of a collective need (the White Paper method). Then the whole war expenditure is part of the National Income of the war years. Even if war expenditure *is* regarded as " costs of production ", it might be debited entirely to the war years on the assumption that it is just in these years that—owing to the aggressive nature of neighbouring countries, for instance—production has become more difficult and more costly.

Or one might adopt Mr. Stern's proposal. But one must then fully realise that his proposal is not just a statistical improvement but introduces really an important sociological hypothesis. For his proposal, if it has to possess any practical meaning, comes to this: that war is a more or less regularly and necessarily recurring phenomenon, " investment " in which is periodically necessary in order to enjoy a peaceful period of moderate length. This hypothesis may be tenable or it may not. But Mr. Stern never states whether he believes this hypothesis to be true or not. Rather, it seems, has he been misled by keeping too close an analogy between the costs of production of a single firm and what he calls the " cost of production " nature of war expenditure. But both, a firm and society, and expenditure on a machine and war expenditure, are phenomena far too different to allow any direct conclusions to be drawn from analogies.

Public Expenditure in the National Income: A Reply

By **Ernest H. Stern**

THE calculations of the National Income are assuming rapidly increasing importance as yardsticks of the economic potential of national communities. The need for clarity of the concept of the National Income is, therefore, assuming equally increasing importance. At present full clarity has not yet been attained. It was with the end in view of contributing to such clarity that I offered criticism and corrections[1] of the method adopted by the White Paper[2] and Meade–Stone.[3] I had no intention—as I hoped to have stated in the introductory words of my article—to " attack ", or even to detract from the value of, the pioneer work of the authors of the White Paper or other compilers of National Income. In the same spirit I welcome the opportunity of further clarification afforded by Mr. K. W. Rothschild's article and—so I hope—the following reply.

I

Mr. Rothschild assumes as reason for my suggestion to omit the category called " security " expenditure and to include the two other categories, called " comfort expenditure " and " savings ", that the first category " only aids production in the private sector of industry " and that the two latter " provide for the collective needs of citizens or lead to an increase in the assets of the nation ". These are not the reasons and I wish to make it clear from the beginning, because in later parts of his article Mr. Rothschild makes inferences from this assumption, which lead to more confusion. My suggestion deals with services remunerated from Public Expenditure and, therefore, derived from other incomes already recorded or from savings, i.e., from residua of income previously recorded. The distinction is between:—

(*a*) services that help to create (or maintain) the other incomes in the magnitudes which are recorded; and

(*b*) services that are not required for creating (or maintaining) incomes, but enjoyable during the accounting period (comfort) or during later periods (savings).

The first category concerns services which if remunerated directly by its recipients according to the value received instead of by the round-about way of taxes and Public Loans, would be accounted as costs of production, therefore not included in the Net Income of the recipients and not recorded for compilation of the " Net " National Income. The two other categories concern services which if remunerated directly would not be allowed as costs of production.

All three categories contain services that provide for the collective needs of citizens which are, therefore, no criterion of distinction. Without a distinction between services allowable and not allowable as costs of production the very useful concept of the " Net " National Income loses its meaning and the estimates lose their comparableness in time as well as between various nations. The failure to make that distinction leads to double-counting if the " Net " National Income is the aggregate of private incomes before Taxation or Public Borrowings have been deducted, to under-counting if Taxation or Public Borrowings have been deducted before compilation.

I am glad that Mr. Rothschild concedes the need for correction of the double-counting that results from the inclusion in the White Paper aggregate, of incomes derived from Public Expenditure. He claims that the compilers knew their totals to be swollen by double-counting but that the difficulty of sorting out Public Expenditure into the allowable and non-allowable categories made them persuade themselves that all Public Expenditure was of the non-allowable kind. They and Meade–Stone failed to say so, or to give any warning as to the possibility of double-counting being included. Nor is it quite correct to claim this to be " the accepted practice of most writers in this field ". As noted a compiler of National Income as S. Kuznets, omits as producing double-counting, part of income derived from Public Expenditure. The reader is referred to footnote (¹) to page 168 of the May, 1943, issue of Economica. Whether or not the compilers of the White Paper were aware of the inclusion of double-counting, double-counting there is.

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[1] ECONOMICA, May, 1943, p. 166 *seq.*

[2] Cmd. 6347/1942 and Cmd. 6438/1943.

[3] *Economic Journal*, June-September, 1941, p. 216 *seq.*

Nor was there need to despair of their ability to divide the Public Expenditure into the suggested categories. Proof of the possibility of a very detailed and exact sorting out has—implicitly—been supplied a few years ago by two American writers, R. W. Nelson and Donald Jackson, in their joint paper[1] on " Allocation of Benefits from Government Expenditure" although application to " Net " National Income calculations was not their object. British statisticians are capable of similar achievements and of gradually perfecting them.

Mr. Rothschild maintains in his example of the imaginary community of rentiers that the suggested sorting out of the services paid from Public Expenditure is insoluble also on logical grounds, arguing that the " security " expenditure satisfies collective wants and therefore contains elements of both " cost of production " and " comfort " nature. The example serves well to refute his very contention. That society is supposed to receive its whole income from foreign investments or tributes—otherwise the rentiers would have to produce something. Its expenditure on external security cannot be supposed to be made for the pleasure of seeing their sons in battledress, but for the purpose of continually, and without hindrance, imposing their will on their debtors or tributers. Its expenditure on internal order cannot be supposed to be made for the pleasure of attending the pageantry of a law court, but for the purpose of preventing themselves from robbing each other of their share in the income. Though the rentiers act collectively, the costs of their action are not spent on " comfort " but as expense in acquiring and maintaining the income and effecting distribution. They are part of their " cost of production ". They choose the collective way as the most economical one.

Once the principle has been accepted to omit from the annual count of " Net " Income the services of a " cost of production " nature, it follows that provision has to be made for the allocation of not annually recurring " costs of production ". War has proved to be not annually recurring, The allocation of the non-comfort and non-saving type of war expenditure involves, therefore, no " sociological hypothesis ", not more so than estimates of the economic life of industrial equipment or patents. In both cases no more and no less is required than a correct anticipation of future events. In both cases the accountants may err, but errors in no way combat the need for providing for obsolescence.

II

Mr. Rothschild claims that the omission of Indirect Taxes from the National Income styled " at factor cost " is not intended to eliminate double-counting, but is significant on its own merits.[2] If this is so, the White Paper and Meade–Stone present us with two sums of figures, viz. :—

(*a*) a sum of private incomes plus business surpluses at market prices containing a substantial measure of double-counting and, therefore, *not* representing the " Net " National Income of the factors of production at market prices; and

(*b*) the same less an amount equal to Indirect Taxation which, therefore, cannot represent the " Net " National Income of the factors of production either unless by sheer good luck the omission of Indirect Taxation eliminates the double-counting included in (*a*).

What then does represent the " Net " National Income of the factors of production? None other than the money value of goods and services annually available to the National Community for comfort and saving, made so available by its own economic efforts after reserving amounts required for their perpetuance and by the contributions, also after provision for perpetuance, of Foreign Communities to which members of the National Community have rights. If the money value is that of current prices, the aggregate is " at market prices " too.

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[1] In Vol. II of the *Studies in Income and Wealth*, National Bureau of Economic Research, New York, 1938.

[2] Mr. Rothschild also says that " the direct taxes received by the State are, of course, not included in the National Income ". This sentence is not understandable after Mr. Rothschild's admission of double-counting and in face of the fact that the White Paper adds up private incomes before Direct Taxes have been deducted and therefore does include them to the amount as they form the income of civil servants and other beneficiaries of Public Expenditure.

Certainly everybody is at liberty to add up certain items and deduct or omit others. Such balances can be multiplied at will. But have they economic significance? Do they represent the " Net " National Income? No. Styling one such balance " Net at factor cost " and another " Net at market prices " conjures up a significance and a distinction too, which neither possess. Is the verdict then that the White Paper Totals have no economic significance? Not quite. By sheer good luck the omission of Indirect Taxation did, in 1938, nearly eliminate the double-counting. Indirect Taxation and Public Expenditure of the " cost of production " category were each close to one-half of Total Public Expenditure. The " net at factor cost " of that year is, therefore, close enough to the mark to be most valuable. This luck held no longer good from 1940.

III

Mr. Rothschild also claims that the method of the White Paper prevents the mode of taxation from disturbing the estimates of the National Income. I maintain the opposite. The proof of the pudding lies in the eating. Let us then test our contentions:—

TEST I: Assume the war to have ended, the man-hours spent on military implements, training, fighting, A.R.P., being utilised for the production of goods and services for comfort and saving, but the rate and kind of taxation maintained, borrowing reduced. Actual results: " Net " National Income greatly increased; White Paper methods: No change of N.N.I.; E.S. method: N.N.I. greatly increased (corresponding to the reduction of $-\left(Sr+\frac{\Sigma Sn}{Q}\right)$ if appropriate war expenditure written off in current years; less greatly, if over longer period.

TEST II: Same assumption, and Direct Taxation reduced. Actual Result: N.N.I. greatly increased; White Paper: No change of N.N.I. E.S. method: N.N.I. increased as in Test I.

TEST III: Same assumption, but Direct Taxation maintained and Indirect Taxation reduced, leading to a decline of the price level. Actual Result: Real N.N.I. greatly increased; White Paper " at factor cost ": Real N.N.I. increased; White Paper " at market prices ": Nominal N.N.I. reduced; Real N.N.I. little change. E.S. method: Real N.N.I. increased as in Test I. Potential behaviour of price level following reduction of Indirect Taxes throws up additional problems which cannot here be elaborated.

TEST IV: Assume peacetime society maintaining military and civil organisation for protection against disturbance from without and within, the expenditure on which is recurring. Assume also gradual spreading of peaceful conditions everywhere leading to reduction of man-hours spent on provisions for external and internal security, the man-hours saved being turned to producing goods or services bought in the market; reduced Public Expenditure leading to reduced Direct Taxation. Actual Result: Increase of N.N.I. White Paper Method: No change. E.S. method: Increase of N.N.I.

TEST V: Assume democratic society with full employment changed over to political tyranny; large numbers withdrawn from private sector recruited to Tyrant's private army for maintaining Tyrant in power, paid from Direct Taxation at higher rates than earned in private sector; Price Level maintained. Actual Result: Reduced N.N.I. White Paper method: Increased N.N.I. E.S. method: Reduced N.N.I.

TEST VI: Similar assumption, but large unemployment before change over, unemployed living on dole, charity (transfer incomes) or savings. Numbers finding employment equal to recruitment to Tyrant's army. Actua Result: No change of N.N.I. White Paper method: Increase of N.N.I. E.S. method: No change of N.N.I.

There is no more space to continue, but readers will greatly profit from doing so. So far the White Paper method has proved to lead to wrong results. If on further testing flaws were found in the E.S. method, the need for correcting the White Paper method would still be proved.

Reviewing the controversy I find that the difference of our respective opinions springs largely from a difference of concepts of the " Net " National Income. That difference is brought out in Section II. I contend that there is no other concept of " *Net* " National Income than claimed there (or in the May issue). There are more concepts of aggregate economic efforts, such as the Gross National Product, the aggregate personal money incomes and others which can claim economic significance. The concepts chosen by the White Paper and Meade–Stone fail to do so and to represent the " *Net* " aggregate Incomes of the National Community.

Scientism and the Study of Society

Part III[1]

xi. “Purposive” Social Formations; xii. “Conscious” Direction and the Growth of Reason; xiii. Engineers and Planners; xiv. Conclusion.

By F. A. v. Hayek

XI

In the concluding portions of this article we have to consider certain practical attitudes which spring from the theoretical views already discussed. Their most characteristic common feature is a direct result of the inability, caused by the lack of a compositive theory of social phenomena, to grasp how the independent action of many men can produce coherent wholes, persistent structures of relationships which serve important human purposes without having been designed for that end. This produces a “pragmatic”[2] interpretation of social institutions which treats all social structures which serve human purposes as the result of deliberate design and denies the possibility of an orderly or purposeful arrangement in anything which is not thus constructed.

This view receives strong support from the fear of employing any anthropomorphic conceptions, so characteristic of the scientistic attitude. This fear has produced an almost complete ban on the use of the concept of “purpose” in the discussion of spontaneous social growths, and it often drives positivists into an error similar to that they wish to avoid: having learnt that it is erroneous to regard everything that behaves in an apparently purposive manner as created by a designing mind, they are led to believe that no result of the action of many men can show order or serve a useful purpose unless it is the result of deliberate design. They are thus driven back to a view which is essentially the same as that which, till the eighteenth century, made man think of language or the family as having been “invented” or the state having been created by an explicit social contract, and in opposition to which the compositive theories of social structures were developed.

As the terms of ordinary language are somewhat misleading, it is necessary to move with great care in any discussion of the “purposive” character of spontaneous social formations. The risk of being lured into an illegitimate anthropomorphic use of the term purpose is as great as that of denying that the term purpose in this connection designates something of importance. In its strict original meaning “purpose” indeed presupposes an acting person deliberately aiming at a result. The same, however, as we have seen before,[3] is true of other concepts like “law” or “organisation”, which we have nevertheless been constrained, by the lack of other suitable terms, to adopt for scientific use in a non-anthropomorphic sense. In the same way we may find the term “purpose” indispensable in a carefully defined sense.

The character of the problem may usefully first be described in the words of an eminent contemporary philosopher who, though elsewhere, in the strict positivist manner, he declares that “the concept of purpose must be entirely excluded from the scientific treatment of the phenomena of life”, yet admits the existence of “a general principle which proves frequently valid in psychology and biology and also elsewhere: namely that the result of unconscious or instinctive processes is frequently exactly the same as would have arisen from rational calculation”.[4] This states one aspect of the problem very clearly: namely, that a result, if it were deliberately aimed at, could be achieved only in a limited number of ways, and that it is actually achieved by one of those methods, although nobody has consciously aimed at it. But it still leaves open the question why the particular result which is brought about in this manner should be regarded as distinguished above others as desirable and therefore deserve to be described as the " purpose ".

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[1] The first two parts of this article appeared in Economica for August, 1942, and February, 1943.

[2] On this concept of the “pragmatic” interpretation of social institutions as for the whole of this section compare Carl Menger, *Untersuchungen über die Methode der Sozialwissenschaften*, 1883 (L.S.E. reprint 1933), book II, chapter 2, which is still the most comprehensive and most careful survey known to me of the problems here discussed.

[3] See Part I of this article, Economica, August, 1942, p. 721.

[4] Cf. M. Schlick, *Fragen der Ethik*, Vienna, 1930, p. 72.

If we survey the different fields in which we are constantly tempted to describe phenomena as " purposive " though they are not directed by a conscious mind, it becomes rapidly clear that the " end " or " purpose " they are said to serve is always the preservation of a " whole ", of a persistent structure of relationships, whose existence we have come to take for granted before we understood the nature of the mechanism which holds the parts together. The most familiar instances of such wholes are the biological organisms. Here the conception of the " function " of an organ as an essential condition for the persistence of the whole has proved to be of the greatest heuristic value. It is easily seen how paralysing an effect on research it would have had if the scientific prejudice had effectively banned the use of all teleological concepts in biology and, e.g., prevented the discoverer of a new organ from immediately asking what " purpose " or " function " it serves.[1]

Though in the social sphere we meet phenomena which in this respect raise analogous problems, it is, of course, dangerous to describe them for that reason as organisms. The limited analogy provides as such no answer to the common problem, and the loan of an alien term tends to obscure the equally important differences. We need not labour here the now familiar facts that the social wholes, unlike the biological organisms, are not given to us as natural units, fixed complexes which ordinary experience shows us to belong together, but are recognisable only by a process of mental reconstruction; or that the parts of the social wholes, unlike those of a true organism, can exist away from their particular place in the whole and are to a large extent mobile and exchangeable. Yet, though we must avoid overworking the analogy, certain general considerations apply equally in both cases. As in the biological organisms so in spontaneous social formations we often observe the parts to move as if their purpose were the preservation of the wholes. We find again and again that *if* it were somebody's deliberate aim to preserve the structure of those wholes, and *if* he had the knowledge and the power to do so, he would have to do it by causing precisely those movements which in fact are taking place without any such conscious direction.

In the social sphere these spontaneous movements which preserve a certain structural connection between the parts are, moreover, connected in a special way with our individual purposes: the social wholes which are thus maintained are the condition for the achievement of many of the things we individually aim at, the environment which makes it possible even to conceive of most of our individual desires and which gives us the power to achieve them.

There is nothing more mysterious in the fact that, e.g., money or the price system enable man to achieve things which he desires, although they were not designed for that purpose, and hardly could have been consciously designed before that growth of civilisation which they made possible, than that, unless man had tumbled upon these devices, he would not have achieved the powers he has gained. The facts to which we refer when we speak of " purposive " forces being here at work are the same as those which create the persistent social structures which we have come to take for granted and which form the condition of our existence. The spontaneously grown institutions are " useful " because they were the conditions on which the further development of man was based—which gave him the powers which he used. If in the form in which Adam Smith put it the phrase that man in society " constantly promotes ends which are no part of his intention " has become the constant source of irritation of the scientistically-minded, it describes nevertheless the central problem of the social sciences. As it was put a hundred years after Smith by Carl Menger, who did more than any other writer to carry beyond Smith the elucidation of the meaning of this phrase, the question " how it is possible that institutions which serve the common welfare and are most important for its advancement can arise without a common will aiming at their creation " is still " the significant, perhaps the most significant, problem of the social sciences ".[2]

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[1] On the use of teleological concepts in biology compare the careful discussion in J. H. Woodger, *Biological Principles*, 1929, particularly the section on " Teleology and Causation ", pp. 429–451; also the earlier discussion in the same work (p. 291) on the so-called " scientific habit of thought " causing the " scandal " of biologists not taking organisation seriously and " in their haste to become physicists, neglecting their business ".

[2] *Untersuchungen, etc.*, p. 163: " Hier ist es wo uns das merkwuerdige, vielleicht das merkwuerdigste Problem der Sozialwissenschaften entgegentritt: Wieso vermoegen dem Gemeinwohl dienende und fuer dessen Entwicklung hoechst bedeutsame Institutionen ohne einen auf ihre Begruendung gerichteten

That the nature and even the existence of this problem is still so little recognised[1] is closely connected with a common confusion about what we mean when we say that human institutions are made by man. Though in a sense man-made, i.e., entirely the result of human actions, they may yet not be designed, not be the intended product of these actions. The term institution itself is rather misleading in this respect, as it suggests something deliberately instituted. It would probably be better if this term were confined to particular contrivances, like particular laws and organisations, which have been created for a specific purpose, and if a more neutral term like " formations " (in a sense similar to that in which the geologists use it, and corresponding to the German *Gebilde*) could be used for those phenomena, which, like money or language, have not been so created.

From the belief that nothing which has not been consciously designed can be useful or even essential to the achievement of human purposes, it is an easy transition to the belief that since all " institutions " have been made by man, we must have complete power to refashion them in any way we desire.[2] But, though this conclusion at first sounds like a self-evident commonplace, it is, in fact, a complete *non sequitur*, based on the equivocal use of the term " institution ". It would be valid only if all the " purposive " formations were the result of design. But phenomena like language or the market, money or morals, are not real artifacts, products of deliberate creation.[3] Not only have they not been designed by any mind, but they are also preserved by, and depend for their functioning on, the actions of people who are not guided by the desire to keep them in existence. And as they are not due to design but rest on individual actions which we do not now control, we can at least not take it for granted that we can improve upon, or even equal, their performance by any organisation which relies on the deliberate control of the movements of its parts. In so far as we learn to understand the spontaneous forces, we may hope to use them and modify their operations by proper adjustment of the institutions which form part of the larger process. But there is all the difference between thus utilising and influencing spontaneous processes and an attempt to replace them by a mechanism relying on conscious control.

We flatter ourselves undeservedly if we represent human civilisation as entirely the product of conscious reason or as the product of human design, or when we assume that it is necessarily in our power deliberately to re-create or to maintain what we have built without knowing what we were doing. Though our civilisation is the result of a cumulation of individual knowledge, it is not by the explicit or conscious combination of all this knowledge in any individual brain, but by its embodiment in symbols which we use without understanding them, in habits and institutions, tools and concepts,[4] that man in society is constantly able to profit from a body of knowledge neither he nor any other man completely possesses. Many of the greatest things man has achieved are not the result of consciously directed thought, and still less the product of a deliberately co-ordinated effort of many individuals, but of a process in

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Gemeinwillen zu entstehen ? " If for the ambiguous and somewhat question-begging term " social welfare " we substitute in this statement " institutions which are necessary conditions for the achievement of man's conscious purposes " it is hardly saying too much that the way in which such " purposive wholes " are formed and preserved is *the* specific problem of social theory, just as the existence and persistence of organisms is the problem of biology.

[1] How much intellectual progress has been obstructed here by political passions is readily seen when we compare the discussion of the problem in the economic and political science with, say, the study of language where, what in the former is still disputed, is a commonplace nobody dreams of questioning.

[2] Menger speaks in this connection rightly of " a pragmatism which, against the wishes of its representatives, leads inevitably to socialism ". (*Untersuchungen, etc.*, p. 208.) To-day this view is most frequently found in the writings of the American " Institutionalists " of which the following (taken from Professor W. H. Hamilton's article on " Institution " in the *Encyclopædia of the Social Sciences*, Vol. VIII, pp. 87–89) is a good example : " The tangled thing called capitalism was never created by design or cut to a blueprint ; but now that it is here, contemporary schoolmen have intellectualised it into a purposive and self-regulating instrument of general welfare ". From this it is only a few steps to the demand " that order and direction should be imposed upon an unruly society ".

[3] A typical example of the treatment of social institutions as if they were true artifacts, in a characteristic scientistic setting, is provided by J. Mayer, *Social Science Principles in the Light of Scientific Method*, Durham, N. C., 1941, p. 20, where society is explicitly " designated as an ' artificial creation ', much as an automobile or steel mill is, that is to say, made by the artifice of man ".

[4] The best illustration, perhaps, of how we constantly make use of the experience or knowledge acquired by others, is the way in which, by learning to speak, we learn to classify things in a certain manner without acquiring the actual experiences which have led successive generations to evolve this system of classification. There is a great deal of knowledge which we never consciously know implicit in the knowledge of which we are aware, knowledge which yet constantly serves us in our actions, though we can hardly be said to " possess " it.

which the individual plays a part which he can never fully understand. They are greater than any individual precisely because they result from the combination of knowledge more extensive than a single mind can master.

It has been unfortunate that those who have recognised this so often draw the conclusion that the problems it raises are purely historical problems, and thereby deprive themselves of the means of effectively refuting the views they try to combat. In fact, as we have seen,[1] much of the older " historical school " was essentially a reaction against the type of erroneous rationalism we are discussing. If it failed it was because it treated the problem of explaining these phenomena as entirely one of the accidents of time and place and refused systematically to elaborate the logical process by which alone we can provide an explanation. We need not return here at any length to this point already discussed.[2] Though the explanation of the way in which the parts of the social whole depend upon each other will often take the form of a genetic account, this will be at most " schematic history " which the true historian will rightly refuse to recognise as real history. It will deal, not with the particular circumstances of an individual process, but only with those steps which are essential to produce a particular result, with a process which, at least in principle, may be repeated elsewhere or at different times. As all explanations, it must run in generic terms, it will deal with what is sometimes called the " logic of events ", neglect much that is important in the unique historical instance, and be concerned with a dependence of the parts of the phenomenon upon each other which is not even necessarily the same as the chronological order in which they appeared. In short, it is not history, but compositive social theory.

One curious aspect of this problem that is rarely appreciated is that it is only by the individualist or compositive method that we can give a definite meaning to the much abused phrases about the social processes and formations being in any sense " more " than " merely the sum " of their parts, and that we are enabled to understand how structures of interpersonal relationships emerge, which make it possible for the joint efforts of individuals to achieve desirable results no individual could have planned or foreseen. The collectivist, on the other hand, who refuses to account for the wholes by systematically following up the interactions of individual efforts, and who claims to be able directly to comprehend social wholes as such, is never able to define the precise character of these wholes or their mode of operation, and is regularly driven to conceive of these wholes on the model of an individual mind.

Even more significant of the inherent weakness of the collectivist theories is the extraordinary paradox that from the assertion that society is in some sense " more " than merely the aggregate of all individuals their adherents regularly pass by a sort of intellectual somersault to the thesis that in order that the coherence of this larger entity be safeguarded it must be subjected to conscious control, i.e., to the control of what in the last resort must be an individual mind. It thus comes about that it is in practice regularly the theoretical collectivist who extols individual reason and demands that all forces of society be made subject to the direction of a single mastermind, while it is the individualist who recognises the limitations of the powers of individual reason and consequently advocates freedom as a mean to the fullest development of the powers of the inter-individual process.

XII

The universal demand for " conscious " control or direction of social processes is one of the most characteristic features of our generation. It expresses perhaps more clearly than any of its other clichés the peculiar spirit of the age. That anything is not consciously directed as a whole is regarded as itself a blemish, a proof of its irrationality and of the need completely to replace it by a deliberately designed mechanism. Yet few of the people who use the term " conscious " so freely seem to be aware what it precisely means; most people seem to forget that " conscious " and " deliberate " are terms which have meaning only when applied to individuals, and that the demand for conscious control is therefore equivalent to the demand for control by a single mind.

This belief that processes which are consciously directed are necessarily superior to any spontaneous process is an unfounded superstition. It would be truer to say as Professor Whitehead has argued in another connection, that on the contrary " civilisation advances by extending the number of important operations we can perform without thinking about them." [1] If it is true that the spontaneous interplay of social forces sometimes solves problems no individual mind could consciously solve, or perhaps even perceives, and if they thereby create an ordered structure which increases the power of the individuals without having been designed by any one of them, they are superior to conscious action. Indeed, any social processes which deserve to be called "social" in distinction to the action of individuals are almost *ex definitione* not conscious. In so far as such processes are capable of producing a useful order which could not have been produced by conscious direction, any attempt to make them subject to such direction would necessarily mean that we restrict what social activity can achieve to the inferior capacity of the individual mind.[2]

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[1] See Part II of this article, ECONOMICA, February, 1943, pp. 50 *et seq.*

[2] *Ibid.*, pp. 54–58. Cf. also Part I, p. 289, and Menger, *Untersuchungen, etc.*, pp. 164 *et seq.*

The full significance of this demand for universal conscious control will be seen most clearly if we consider it first in its most ambitious manifestation, even though this is as yet merely a vague aspiration and important mainly as a symptom: this is the application of the demand for conscious control to the growth of the human mind itself. This audacious idea is the most extreme result to which man has yet been led by the success of reason in the conquest of external nature. It has become a characteristic feature of contemporary thought and appears in what on a first view seem to be altogether different and even opposite systems of ideas. Whether it is the late L. T. Hobhouse who holds up to us "the ideal of a collective humanity self-determining in its progress as the supreme object of human activity and the final standard by which the laws of conduct should be judged",[3] or Dr. Joseph Needham who argues that "the more control consciousness has over human affairs, the more truly human and hence super-human man will become",[4] whether it is the strict followers of Hegel who adumbrate the master's view of Reason becoming conscious of itself and taking control of its fate, or Dr. Karl Mannheim who thinks that "man's thought has become more spontaneous and absolute than it ever was, since it now perceives the possibility of determining itself",[5] the basic attitude is the same. Though, according as these doctrines spring from Hegelian or positivist views, those who hold them form distinct groups who mutually regard themselves as completely different from and greatly superior to the other, the common idea that the human mind is, as it were, to pull itself up by its own boot-straps, springs from the same general approach: the belief that by studying human Reason from the outside and as a whole we can grasp the laws of its motion in a more complete and comprehensive manner than by its patient exploration from the inside, by actually following up the processes in which individual minds interact.

This pretension to be able to increase the powers of the human mind by consciously controlling its growth is thus based on the same theoretical view which claims to be able fully to explain this growth, a claim which implies the possession of a kind of super-mind on the part of those who make it; and it is no accident that those who hold these theoretical views should also wish to see the growth of mind thus directed.

It is important to understand the precise sense in which the claim to be able to "explain" existing knowledge and beliefs must be interpreted in order to justify the aspirations based on it. For this purpose it would not be sufficient if we possessed an adequate theory which explained the *principles* on which the processes operate to which the growth of mind is due. Such knowledge of the mere principles (either a theory of knowledge or a theory of the social processes involved) might assist in creating conditions favourable to that growth, but could never provide a justification for the claim to direct it. This claim presupposes that we are able to arrive at a substantive explanation of why we hold the particular views we hold, of how our actual knowledge is determined by specific conditions. It is this which the "sociology of knowledge" and the various other derivatives of the "materialist interpretation of history" undertake when, e.g., they "explain" the Kantian philosophy as the product of the material interests of the German bourgeoisie in the late 18th century, or whatever other similar theses they present. We cannot enter here into a discussion of the reasons why even with respect to views now regarded as errors, and which on the basis of our better present knowledge we may in a sense be able to explain, that method does not really provide an explanation. The crucial point is that to attempt this with respect to our present knowledge involves a contradiction: if we knew how our present knowledge is conditioned or determined, it would no longer be our present knowledge. To assert that we can explain our own knowledge is to assert that we know more than we do know, a statement which is non-sense in the strict meaning of that term.[1] There may, perhaps, be sense in the statement that to a greatly superior mind our present knowledge would appear as "relative" or conditioned in a certain manner by assignable circumstances. But the only conclusion *we* should be entitled to draw from this would be one opposite to that of the "boot-strap theory of mental evolution"; it would be that on the basis of our present knowledge we are not in a position successfully to direct its growth. To draw any other conclusion than this, to derive from the thesis that human beliefs are determined by circumstances the claim that anyone should be given power to determine these beliefs, involves the claim that those who are to assume that power possess some sort of super-mind. Those who hold these views have indeed regularly some special theory which exempts their own views from the same sort of explanation and credits them, as a specially favoured class, or simply as the "free-floating intelligentsia", with the possession of absolute knowledge.

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[1] A. N. Whitehead, *An Introduction to Mathematics* (Home University Library), 1911, p. 61.

[2] It cannot be objected to this that what is meant by conscious control is not control by a single mind but by a concerted and "co-ordinated" effort of all, or all the best minds instead of by their fortuitous interplay. This phrase about the deliberate co-ordination merely shifts the task of the individual mind to another stage but leaves the ultimate responsibility still with the co-ordinating mind. Committees and other devices for facilitating communications are excellent means to assist the individual in learning as much as possible; but they do not extend the capacity of the individual mind. The knowledge that can be consciously co-ordinated in this manner is still limited to what the individual mind can effectively absorb and digest. As every person with experience of committee work knows, its fertility is limited to what the best mind among the members can master; if the results of the discussion are not ultimately turned into a coherent whole by an individual mind, they are likely to be inferior to what would have been produced unaided by a single mind.

[3] L. T. Hobhouse, *Democracy and Reaction*, 1904, p. 108.

[4] J. Needham, *Integrative Levels. A Revaluation of the Idea of Progress* (Herbert Spencer Lecture), Oxford, 1937, p. 47.

[5] K. Mannheim, *Man and Society in an Age of Reconstruction*, 1940, p. 213.

While in a sense this movement represents thus a sort of super-rationalism, a demand for the direction of everything by a super-mind, it prepares at the same time the ground for a thorough irrationalism. If truth is no longer discovered by observation, reasoning and argument, but by uncovering hidden causes which, unknown to the thinker, have determined his conclusions, if whether a statement is true or false is no longer decided by logical argument and empirical tests, but by examining the social position of the person who made it, when in consequence it becomes the membership of a class or race which secures or prevents the achievement of truth, and when in the end it is claimed that the sure instinct of a particular class or a people is always right, reason has been finally driven out.[2] This is no more than the natural result of a doctrine which starts out with the claim that it can intuitively recognise wholes in a manner superior to the rational reconstruction attempted by compositive social theory.

If it is true, moreover, as in their different ways both individualists and collectivists contend, that social processes can achieve things which it is beyond the power of the individual mind to achieve and plan, and that it is from those social processes that the individual mind derives what power it possesses, the attempt to impose conscious control on these processes must have even more fatal consequences. The presumptuous aspiration that "reason" should direct its own growth could in practice only have the effect that it would set limits to its own growth, that it would confine itself to the results which the directing individual mind can already foresee. Though this aspiration is a direct outcome of a certain brand of rationalism, it is, of course, the result of a misunderstood or misapplied rationalism which fails to recognise the extent to which individual reason is a product of inter-individual relationships. Indeed, the demand that everything, including the growth of mind, should be consciously controlled, is itself a sign of the inadequacy of the understanding of even the general character of the forces which constitute the life of the human mind and of human society. It is the extreme stage of these self-destructive forces of our modern "scientific" civilisation, of that abuse of reason whose development and consequences will be the central theme of the following historical studies.

It is because the growth of the human mind presents in its most general form the common problem of all the social sciences that it is here that minds most sharply divide, and two fundamentally different and irreconcilable attitudes manifest themselves. On the one hand the essential humility of individualism, which endeavours to understand as well as possible the principles by which the efforts of individual men have in fact been combined to produce our civilisation, and from this understanding hopes to derive the power to create conditions favourable to further growth; and, on the other hand, the hybris of collectivism which aims at conscious direction of all forces of society. The individualist approach, in awareness of the constitutional limitations of the individual mind,[1] attempts to show how man in society is able, by the use of various resultants of the social process, to increase his powers with the help of the knowledge implicit in them and of which he is never aware; it makes us understand that the only " reason " which can in any sense be regarded as superior to individual reason does not exist apart from the inter-individual process in which, by means of impersonal media, the knowledge of successive generations and of millions of people living simultaneously is combined and mutually adjusted, and that this process is the only form in which the totality of human knowledge ever exists. The collectivist method, on the other hand, not satisfied with the partial knowledge of this process from the inside, which is all the individual can gain, bases its demand for conscious control on the assumption that it can comprehend this process as a whole and make use of all knowledge in a systematically integrated form. It leads thus directly to political collectivism; and though logically methodological and political collectivism are distinct, it is not difficult to see how the former leads to the latter and how, indeed, without methodological collectivism political collectivism would be deprived of its intellectual basis: without the pretension that conscious individual reason can grasp all the aims and all the knowledge of " society " or " humanity ", the belief that these aims are best achieved by conscious central direction loses its foundation. Consistently pursued it must lead to a system in which all members of society become merely instruments of the single directing mind and in which all the spontaneous social forces to which the growth of the mind is due are destroyed.[2]

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[1] See Part II of this article, ECONOMICA, February, 1943, pp. 36 and 60 *seq.*

[2] Interesting illustrations of the length to which these absurdities have been carried will be found in E. Gruenwald, *Das Problem der Soziologie des Wissens*, Vienna, 1934, a posthumously published sketch of a very young scholar which still constitutes the most comprehensive survey of the literature of the subject.

It may indeed prove to be far the most difficult and not the least important task for human reason rationally to comprehend its own limitations. It is essential for the growth of reason that as individuals we should bow to forces and obey principles which we cannot hope fully to understand yet on which the advance and even the preservation of civilisation depends.[3] Historically this has been achieved by the influence of the various religious creeds and by traditions and superstitions which made men submit to those forces by an appeal to his emotions rather than his reason. The most dangerous stage in the growth of civilisation may well be that in which man has come to regard all these beliefs as superstitions and refuses to accept or to submit to anything which he does not rationally understand. The rationalist whose reason is not sufficient to teach him those limitations of the powers of conscious reason, and who despises all the institutions and customs which have not been consciously designed, would thus become the destroyer of the civilisation built upon them. This may well prove a hurdle which man will repeatedly reach, only to be thrown back into barbarism.

It would lead too far here more than briefly to refer to another field in which this same characteristic tendency of our age shows itself: that of morals. Here it is against the observance of any general and formal rules whose *rationale* is not explicitly demonstrated that the same kind of objections are raised. But the demand that every action should be judged after full consideration of all its consequences and not by any general rules is due to a failure to see that the submission to general rules, couched in terms of immediately ascertainable circumstances, is the only way in which for man with his limited knowledge freedom can be combined with the essential minimum degree of order. Common acceptance of formal rules is indeed the only alternative to direction for a common purpose man has yet discovered. The general acceptance of such a body of rules is no less important because they have not been rationally constructed. It is at least doubtful whether it would be possible in this way to construct a new moral code that would have any chance of acceptance. But so long as we have not succeeded in doing so, any general refusal to accept existing moral rules merely because their expediency has not been rationally demonstrated (as distinguished from the case when the critic believes he has discovered a better moral rule in a particular instance and is willing to brave public disapproval in testing it) is to destroy one of the roots of our civilisation.[4]

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[1] Cf. Part I of this article, ECONOMICA, August, 1942, pp. 280 and 290.

[2] It is, perhaps, not so obvious as to make it unnecessary to mention it, that the fashionable disparagement of any activity which, in science or the arts, is carried on " for its own sake ", and the demand for a " conscious social purpose " in everything, is an expression of the same general tendency and based on the same illusion of complete knowledge as those discussed in the text.

[3] Some further aspects of the big problems here just touched upon are discussed in my *Road to Serfdom*, 1944, particularly chapters VI and XIV.

[4] It is characteristic of the spirit of the time, and of positivism in particular, when A. Comte speaks (*Système de Politique Positive*, Vol. I, p. 356) of " La supériorité necessaire de la morale démontrée sur la morale révélée ", characteristic especially in its implied assumption that a rationally constructed moral system is the only alternative to one revealed by a higher being.

XIII

The ideal of conscious control of social phenomena has made its greatest influence felt in the economic field. The present popularity of " economic planning " is directly traceable to the prevalence of the scientistic ideas we have been discussing. As in this field the scientistic ideals manifest themselves in the particular forms which they take in the hands of the applied scientist and especially the engineer, it will be convenient to combine the discussion of this influence with some examination of the characteristic ideals of the engineers. We shall see that the influence on current views about problems of social organisation of his technological approach, or the engineering point of view, is much greater than is generally realised. Most of the schemes for a complete remodelling of society, from the earlier utopias to modern socialism, bear indeed the distinct mark of this influence. In recent years this desire to apply engineering technique to the solution of social problems has become very explicit;[1] " political engineering " and " social engineering " have become fashionable catchwords which are quite as characteristic of the outlook of the present generation as its predilection for " conscious " control; in Russia even the artists appear to pride themselves on the name of " engineers of the soul ", bestowed upon them by Stalin. These phrases suggest a confusion about the fundamental differences between the task of the engineer and that of social organisations on a larger scale which make it desirable to consider their character somewhat more fully.

We must confine ourselves here to a few salient features of the specific problems which the professional experience of the engineer constantly bring up and which determine his outlook. The first is that his characteristic tasks are usually in themselves complete: he will be concerned with a single end, control all the efforts directed towards this end, and dispose for this purpose of a definitely given supply of resources. It is as a result of this that the most characteristic feature of his procedure becomes possible, namely that, at least in principle, all the parts of the complex of operations are preformed in the engineer's mind before they start, that all the " data " on which the work is based have explicitly entered his preliminary calculations and been condensed into the " blue-print " that governs the execution of the whole scheme.[2] [3] The engineer, in other words, has complete control of the particular little world with which he is concerned, surveys it in all its relevant aspects and has to deal only with " known quantities ". So far as the solution of his engineering problem is concerned, he is not taking part in a social process in which others may take independent decisions, but lives in a separate world of his own. The application of the technique which he has mastered, of the generic rules he has been taught, indeed presupposes such complete knowledge of the objective facts; those rules refer to objective properties of the things and can be applied only after all the particular circumstances of time and place have been assembled and brought under the control of a single brain. His technique, in other words, refers to typical situations defined in terms of objective facts, not to the problem of how to find out what resources are available or what is the relative importance of different needs. He has been trained in objective possibilities, irrespective of the particular conditions of time and place, in the knowledge of those properties of things which remain the same everywhere and at all times and which they possess irrespective of a particular human situation.

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[1] Once again one of the best illustrations of this tendency is provided by K. Mannheim, *Man and Society in an Age of Reconstruction*, 1940, particularly pp. 240–244, where he explains that " functionalism made its first appearance in the field of the natural sciences, and could be described as the technical point of view. It has only recently been transferred to the social sphere . . . Once this technical approach was transferred from natural sciences to human affairs, it was bound to bring about a profound change in man himself . . . The functional approach no longer regards ideas and moral standards as absolute values, but as products of the social process which can, if necessary, be changed by scientific guidance combined with political practice . . . The extension of the doctrine of technical supremacy which I have advocated in this book is in my opinion inevitable . . . Progress in the technique of organisation is nothing but the application of technical conceptions to the forms of co-operation. A human being, regarded as part of the social machine, is to a certain extent stabilised in his reactions by training and education, and all his recently acquired activities are co-ordinated according to a definite principle of efficiency within an organised framework."

[2] The best description of this feature of the engineering approach by an engineer which I have been able to find occurs in a speech of the great German optical engineer Ernst Abbe: " Wie der Architekt ein Bauwerk, bevor eine Hand zur Ausführung sich rührt, schon im Geist vollendet hat, nur unter Beihilfe von Zeichenstift und Feder zur Fixierung seiner Idee, so muss auch das komplizierte Gebilde von Glas und Metal sich aufbauen lassen rein verstandesmässig, in allen Elementen bis ins letzte vorausbestimmt, in rein geistiger Arbeit, durch theoretische Ermittlung der Wirkung aller Teile, bevor diese Teile noch körperlich ausgeführt sind. Der arbeitenden Hand darf dabei keine andere Funktion mehr verbleiben als die genaue Verwircklichung der durch die Rechnungen bestimmten Formen und Abmessungen aller Konstruktionselemente, und der praktischen Erfahrung keine andere Aufgabe als die Beherrschung der Methoden und Hilfsmittel, die für letzteres, die körperliche Verwirklichung, geeignet sind " (quoted by Franz Schnabel, *Deutsche Geschichte im neunzehnten Jahrhundert*, vol. III, 1934, p. 222—a work which is a mine of information on this as on all other matters of the intellectual history of Germany in the nineteenth century).

[3] It would take too long here to explain in any detail why, whatever delegation or division of labour is possible in preparing an engineering ' blueprint ', is very limited and differs in essential respects from the division of knowledge on which the impersonal social processes rest. It must suffice to point out that not only must the precise nature of the result be fixed which anyone who has to draw up part of an engineering plan must achieve, but also that, in order to make such delegation possible, it must be known that the result can be achieved at no more than a certain maximum cost.

It is important, however, to observe that the engineer's view of his job as complete in itself is, in some measure, a delusion. He is in a position to treat it as such in a competitive society because he can regard the assistance from society at large on which he counts as one of his data, as given to him without having to bother about it. That he can buy at given prices the materials and the services of the men he needs, that if he pays his men they will be able to procure their food and other necessities, he will usually take for granted. It is through basing his plans on the data offered to him by the market that they are fitted into the larger complex of social activities; and it is because he need not concern himself how the market provides him with what he needs that he can treat his job as self-contained. So long as market prices do not change unexpectedly he uses them as a guide in his calculations without much reflection about their significance. But, though he is compelled to take them into account, they are not properties of things of the same kind as those which he understands. They are not objective attributes of things but reflections of a particular human situation at a given time and place. And as his knowledge does not explain why those changes in prices occur which often interfere with his plans, any such interference appears to him due to irrational (i.e., not consciously directed) forces and he resents the necessity of paying attention to magnitudes which appear meaningless to him. Hence the characteristic and ever-recurrent demand for the substitution of *in natura*[1] calculation for the "artificial" calculation in terms of price or value, i.e., of a calculation which takes explicit account of the objective properties of things.

The engineer's ideal which he feels the "irrational" economic forces prevent him from achieving, based on his study of the objective properties of the things, is usually some purely technical optimum of universal validity. He rarely sees that his preference for these particular method its merely a result of the type of problem he has most frequently to solve, and justified only in particular social positions. Since the most common problem the builder of machines meets is to extract from given resources the maximum of power, with the machinery to be used the variable under his control, this maximum utilisation of power is set up as an absolute ideal, a value in itself.[2] But there is, of course, no special merit in economising one of the many factors which limit the possible achievement, at the expense of others. The engineer's "technical optimum" proves frequently to be simply that method which it would be desirable to adopt if the rate of interest were zero—or the supply of capital unlimited, which would indeed be a position in which we would aim at the highest possible rate of transformation of current input into current output. But to treat this as an immediate goal is to forget that such a state can be reached only by diverting for a long time resources which are wanted to serve current needs to the production of equipment. In other words, the engineer's ideal is based on the disregard of the most fundamental economic fact which determines our position here and now, the scarcity of capital.

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[1] The most persistent advocate of such *in natura* calculation is, significantly, Dr. Otto Neurath, the protagonist of modern "physicalism" and "objectivism".

[2] Cf. the characteristic passage in B. Bavinck, *The Anatomy of Modern Science* (translated from the 4th German edition by H. S. Hatfield), 1932, p. 564: "When our technology is still at work on the problem of transforming heat into work in a manner better than that possible with our present-day steam and other heat engines . . ., this is not directly done to cheapen production of energy, but first of all because it is an end in itself to increase the thermal efficiency of a heat engine as much as possible. If the problem set is to transform heat into work, then this must be done in such a way that the greatest possible fraction of the heat is so transformed . . . The ideal of the designer of such machines is therefore the efficiency of the Carnot cycle, the ideal process which delivers the greatest theoretical efficiency".

It is easy to see why this approach, together with the desire to achieve a calculation *in natura*, leads engineers so frequently to the construction of systems of "energetics" that it has been said, with much justice, that "das Characteristikum der Weltanschauung des Ingenieurs ist die energetische Weltanschauung" (L. Brinkmann, *Der Ingenieur*, Frankfurt, 1908, p. 16). We have already referred (Part II of this article, ECONOMICA, February, 1943, p. 40) to this characteristic manifestation of scientistic "objectivism" and there is no space here to return to it in greater detail. But it deserves to be recorded how widespread and typical this view is and how great the influence it has exercised. E. Solvay, G. Ratzenhofer, W. Ostwaldt, P. Geddes, F. Soddy, H. G. Wells, the "Technocrats" and L. Hogben are only a few of the influential authors in whose works "energetics" play a more or less prominent rôle. There are several studies of this movement in French and German (Nyssens, *L'énergetique*, Brussels, 1908; G. Barnich, *Principes de politique positive basée sur l'énergetique sociale de Solvay*, Brussels, 1918; G. Helm, *Die Energetik in ihrer geschichtlichen Entwicklung*, Berlin, 1898; Schnehen, *Energetische Weltanschauung*, 1907,. A. Dochmann, *F. W. Ostwald's Energetik*, Bern 1908, and the best. Max Weber, "Energetische Kulturtheorien" (1909) reprinted in *Gesammelte Aufsätze zur Wissenschaftslehre*, (1922), but none of them adequate and none, to my knowledge, in English.

The section from the work of Bavinck from which a passage has been quoted above condenses the gist of the enormous literature, mostly German, on the "philosophy of technology" which has had a wide circulation and of which the best known is E. Zschimmer. *Philosophie der Technik*, 3rd ed., Stuttgart, 1933.

The rate of interest is, of course, only one, though the least understood and therefore most disliked, of those prices which act as impersonal guides to which the engineer must submit if his plans are to fit into the pattern of activity of society as a whole, and against the restraint of which he chafes because they represent forces whose *rationale* he does not understand. It is one of those symbols in which the whole complex of human knowledge and wants is automatically (though by no means faultlessly) recorded and to which the individual must pay attention if he wants to keep in step with the rest of the system. If, instead of using this information in the abridged form in which it is conveyed to him through the price system, he were to try in every instance to go back to the objective facts and take them consciously into consideration, this would be to dispense with the method which makes it possible for him to confine himself to the immediate circumstances and to substitute for it a method which requires that all this knowledge be collected in one centre and explicitly and consciously embodied in a unitary plan. The application of engineering technique to the whole of society requires indeed that the director possess the same complete knowledge of the whole society that the engineer possesses of his limited world. Central economic planning is nothing but such an application of engineering principles to the whole of society based on the assumption that such a complete concentration of all relevant knowledge is possible.[1]

Before we proceed to consider the significance of this conception of a rational organisation of society it will be useful to supplement the sketch of the typical outlook of the engineer by an even briefer sketch of the functions of the merchant or trader. This will not only further elucidate the nature of the problem of the utilisation of knowledge dispersed among many people, but also help to explain the dislike which not only the engineer but our whole generation shows for all commercial activities, and the general preference that is now accorded to "production" compared to the activities which, somewhat misleadingly, are referred to as "distribution".

Compared with the work of the engineer that of the merchant is in a sense much more "social", i.e., interwoven with the free activities of other people. He contributes a step towards the achievement now of one end, now of another and hardly ever is concerned with the complete process that serves a final need. What concerns him is not the achievement of a particular final result of the complete process in which he takes part, but the best use of the particular means of which he knows. His special knowledge is almost entirely knowledge of particular circumstances of time or place, or, perhaps, a technique of ascertaining those circumstances in a given field. But though this knowledge is not of a kind which can be formulated in generic propositions or acquired once and for all, and though in an age of Science it is for that reason regarded as knowledge of an inferior kind, it is for all practical purposes no less important than scientific knowledge. And while it is perhaps conceivable that all theoretical knowledge might be combined in the heads of a few experts and thus made available to a single central authority, it is this knowledge of the particular, of the

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This literature is very instructive as a psychological study, though otherwise about the dreariest mixture of pretentious platitudes and revolting nonsense which it has ever been the ill fortune of the present author to peruse. Its common feature is the enmity towards all economic considerations, the attempted vindication of purely technological ideals, and the glorification of the organisation of the whole of society on the principle on which a single factory is run. (On the last point see particularly F. Dessauer, *Philosophie der Technik*, Bonn, 1927. p. 129.) It would be unfair, however, not to mention here the brilliant study in which an eminent American engineer has very clearly shown the limitations of the application of engineering technique to problems of social organisation: D. C. Coyle, "The Twilight of National Planning," *Harper's Magazine*, October, 1935.

[1] That this is fully recognised by its advocates is shown by the popularity among all socialists from Saint-Simon to Marx and Lenin, of the phrase that the whole of society should be run in precisely the same manner as a single factory is now being run. Cf. V. I. Lenin, *The State and Revolution* (1917), "Little Lenin Library," 1933, p. 78. "The whole of society will have become a single office and a single factory with equality of work and equality of pay"; and for Saint-Simon and Marx, ECONOMICA, February, 1941, p. 28.

fleeting circumstances of the moment and of local conditions, which will never exist otherwise than dispersed among many people. The knowledge of when a particular material or machine can be most effectively used or where they can be most quickly or cheaply obtained is quite as important for the solution of a particular task as the knowledge of what is the best material or machine for the purpose. The former kind of knowledge has little to do with the permanent properties of classes of things which the engineer studies, but is knowledge of a particular human situation. And it is as the person whose task is to take account of these facts that the merchant will constantly come into conflict with the ideals of the engineer, with whose plans he interferes and whose dislike he thereby contracts.

XIV

The problem of securing an efficient use of our resources is thus very largely one of how that knowledge of the particular circumstances of the moment can be most effectively utilised; and the task which faces the designer of a rational order of society is to find a method whereby this widely dispersed knowledge may best be drawn upon. It is begging the question to describe this task, as is usually done, as one of effectively using the "available" resources to satisfy "existing" needs. Neither the "available" resources nor the "existing" needs are objective facts in the sense of those with which the engineer deals in his limited field: they can never be directly known in all relevant detail to a single planning body. Resources and needs exist for practical purposes only through somebody knowing about them and there will always be infinitely more known to all the people together than can be known to the most competent authority.[1] A successful solution can therefore not be based on the authority dealing directly with the objective facts, but must be based on a method of utilising the knowledge dispersed among all members of society, knowledge of which in any particular instance the central authority will usually know neither who possesses it nor whether it exists at all. It can therefore not be utilised by consciously integrating it into a coherent whole, but only through some mechanism which will delegate the particular decisions to those who possess it, and for that purpose supply them with such information about the general situation as will enable them to make the best use of the particular circumstances of which only they know.

This is precisely the function which the various "markets" perform. Though every party in them will know only a small sector of all the possible sources of supply, or of the uses of, a commodity, yet, directly or indirectly, the parties are so interconnected that the prices register the relevant net result of all changes affecting demand or supply.[2] It is as such an instrument for communicating to all those interested in a particular commodity the relevant information in an abridged and condensed form that markets and prices must be seen if we are to understand their function. They help to utilise the knowledge of many people without the need of first collecting it in a single body, and thereby make possible that combination of decentralisation of decisions and mutual adjustment of these decisions which we find in a competitive system.

In aiming at a result which must be based, not on a single body of integrated knowledge or of connected reasoning which the designer possesses, but on the separate knowledge of many people, the task of social organisation differs fundamentally from that of organising given material resources. The fact that no single mind can know more than a fraction of what is known to all individual minds sets limits to the extent to which conscious direction can improve upon the results of unconscious social processes. Man has not deliberately designed this process and has begun to understand it only long after it had grown up. But that something that not only does not rely on deliberate control for its working, but has not even been deliberately designed, should bring about desirable results, which we might not be able to bring about otherwise, is a conclusion the natural scientist seems to find difficult to accept.

It is because the moral sciences tend to show us such limits to our conscious control, while the progress of the natural sciences constantly extends the range of conscious control, that the natural scientist finds himself so frequently in revolt against the teaching of the moral sciences. Economics, in particular, after being condemned for employing methods different from those of the natural scientist, stands doubly condemned because it claims to show limits to the technique by which the natural scientists continuously extend our conquest and mastery of nature.

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[1] It is important to remember in this connection that the statistical aggregates which it is often suggested the central authority could rely upon in its decisions, are always arrived at by a deliberate disregard of the peculiar circumstances of time and place.

[2] Cf. in this connection the suggestive discussion of the problem in K. F. Mayer, *Goldwanderungen*, Jena, 1935, pp. 66–68, and also the present author's article "Economics and Knowledge" in ECONOMICA, February, 1937.

It is this conflict with a strong human instinct, greatly strengthened in the person of the scientist and engineer, that makes the teaching of the moral sciences so very unwelcome. As Bertrand Russell has well described the position, " the pleasure of planned construction is one of the most powerful motives in men who combine intelligence with energy; whatever can be constructed according to a plan, such man will endeavour to construct . . . the desire to create is not in itself idealistic since it is a form of the love of power, and while the power to create exists there will be men desirous of using this power even if unaided nature would produce a better result than any that can be brought about by deliberate intention ".[1] This statement itself occurs, however, at the beginning of a chapter, significantly headed " Artificially Created Societies ", in which Russell himself seems to support these tendencies by arguing that " no society can be regarded as fully scientific unless it has been created deliberately with a certain structure to fulfil certain purposes ".[2] As this statement will be understood by most readers, it expresses concisely that scientistic philosophy which through its popularisers has done more to create the present trend towards socialism than all the conflicts between economic interests which, though they raise a problem, do not necessarily indicate a particular solution. Of the majority of the intellectual leaders of the socialist movement, at least, it is probably true to say that they are socialists because socialism appears to them, as A. Bebel, the leader of the German Social Democratic movement defined it more than fifty years ago, as " science applied in clear awareness and with full insight to all fields of human activity."[3] The proof that the programme of socialism actually derives from this kind of scientistic philosophy must be reserved for detailed historical studies. At present our concern is mainly to show to what extent mere intellectual error in this field may profoundly affect all prospects of humanity.

What all the people who are so unwilling to renounce any of the powers of conscious control seem to be unable to comprehend is that this renunciation of conscious power, power which must always be power by men over other men, is for society as a whole only an apparent resignation, a self-denial individuals are called upon to exercise in order to increase the powers of the race, to release the knowledge and energies of the countless individuals that could never be utilised in a society consciously directed from the top. The great misfortune of our generation is that the direction which has been given to its interests by the amazing progress of the natural sciences is not one which assists us in comprehending the larger process of which as individuals we form merely a part, and in appreciating how we constantly contribute to a common effort without either directing it or submitting to orders of others. To see this requires a kind of intellectual effort different in character from that necessary for the control of material things, an effort in which the traditional education in the " humanities " gave at least some practice, but for which the now predominant types of education seem less and less to prepare. The more our technical civilisation advances and the more, therefore, the study of things as distinct from the study of men and their ideas qualifies for the more important and influential positions, the more significant becomes the gulf that separates two different types of mind: the one represented by the man whose supreme ambition is to turn the world round him into an enormous machine, every part of which, on his pressing a button, moves according to his design; and the other represented by the man whose main interest is the growth of the human mind in all its aspects, who in the study of history or literature, the arts or the law, has learnt to see the individuals as part of a process in which his contribution is not directed but spontaneous, and where he assists in the creation of something greater than he or any other single mind can ever plan for. It is this awareness of being part of a social process, and of the manner in which individual efforts interact, which the education solely in the Sciences or in technology seems to fail so lamentably to convey. It is not surprising that many of the more active minds among those so trained sooner or later react violently against the deficiencies of their education and develop a passion for imposing on society the order which they are unable to discover by the means with which they are familiar.

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[1] *The Scientific Outlook*, 1931, p. 211.

[2] *Ibid.*, p. 211. The passage quoted could be interpreted in an unobjectionable sense if " certain purposes " is taken to mean not particular predetermined results but as capacity to provide what the individuals at any time wish—i.e., if what is planned is a machinery which can serve many ends and need not in turn be " consciously " directed towards a particular end.

[3] A. Bebel, *Die Frau und der Sozialismus*, 13th ed., 1892, p. 376. "Der Sozialismus ist die mit klarem Bewusstsein and mit voller Erkenntnis auf alle Gebiete menschlicher Taetigkeit angewandte Wissenschaft." Cf. also E. Ferri, *Socialism and Positive Science* (translated from the Italian edition of 1894,. The first clearly to see this connection seems to have been M. Ferraz, *Socialisme, Naturalisme et Positivisme*, Paris, 1877.

In conclusion it is, perhaps, desirable to remind the reader once more that all we have said here is directed solely against a misuse of Science, not against the scientist in the special field where he is competent, but against the application of his mental habits in fields where he is not competent. There is no conflict between our conclusions and those of legitimate science. The main lesson at which we have arrived is indeed the same as that which one of the acutest students of scientific method has drawn from a survey of all fields of knowledge: it is that " the great lesson of humility which science teaches us, that we can never be omnipotent or omniscient, is the same as that of all great religions: man is not and never will be the god before whom he must bow down ".[1]

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[1] M. R. Cohen, *Reason and Nature*, 1931, p. 449. It is significant that one of the leading members of the movement with which we are concerned, the German philosopher Ludwig Feuerbach, explicitly chose the opposite principle, *homo homini Deus*, as his guiding maxim.

Book Reviews

Capitalism, Socialism and Democracy. By JOSEPH SCHUMPETER. Harper Brothers. 1942. x + 381 pp. $3.50.

This is a theme which gives Professor Schumpeter the opportunity to appear at his best. He approaches questions of institutions and economic tendencies from the standpoint of a lively and wide interest in human nature.

The book starts with a critical examination of Marxian doctrine—Marx the Prophet succeeded in " weaving together those extra-rational cravings which receding religion had left running about like masterless dogs, and the rationalistic and materialistic tendencies of the time " (p. 6). Marx the Sociologist " linked the fate of the class phenomenon with the fate of capitalism " (p. 19). Marx the Economist was a follower of Ricardo (p. 22), but not merely a follower. His one truly great achievement was " to see and to teach systematically how economic theory may be turned into historical analysis, and how the historical narrative may be turned into *histoire raisonnée* ", instead of assigning the facts of economic history " to a separate compartment " (p. 44).

There follows Part II, " Can Capitalism Survive ? " Professor Schumpeter has his own individual view of the working of capitalism. The Marxian theory of exploitation, depending on the effect of a perpetual reserve of labour in keeping wages down to the subsistence level, is disproved by experience (pp. 34–7). He himself belongs to the school of thought which treats profit as an excrescence of the economic system, so that in the stationary state neither profit nor interest would exist.

Profit in his view is derived from " innovations ", and he uses the term entrepreneur for one who initiates an innovation. " The fundamental impulse that sets and keeps the capitalist engine in motion comes from the new consumers' goods, the new methods of production or transportation, the new markets, the new forms of industrial organisation, that capitalist enterprise creates " (p. 83). " In dealing with capitalism, we are dealing with an evolutionary process " ; capitalism " is by nature a form or method of economic change, and not only never is but never can be stationary ".

Even monopoly profits are of slight significance, though " there is or may be an element of genuine monopoly gain in those entrepreneurial profits which are the prizes offered by capitalist society to the successful innovator " (p. 102).

" Can capitalism survive ? " Professor Schumpeter asks. " No, I do not think it can," he replies (p. 61).

It might be supposed that, if innovation is " the fundamental impulse that sets and keeps the capitalist engine in motion ", the end would come through an exhaustion of possible innovations. In Chapter X he turns to consider the prospect of a " vanishing of investment opportunity ". In his opinion " there is no reason to expect slackening of the rate of output through exhaustion of technological possibilities " (p. 118), even when " capital-saving " devices predominate (pp. 119–20). But in Chapter XII he poses the possibility that " the economic wants of humanity might be so completely satisfied that little motive would be left to push productive effort still further ahead " (p. 131). If improvements in methods of production were also assumed to have reached their limit, " a more or less stationary state would ensue. . . . There would be nothing left for entrenpreneurs to do. . . . Profits and, along with profits, the rate of interest would converge towards zero. The bourgeois strata that live on profits and interest would tend to disappear."

" For the calculable future," Professor Schumpeter comments, " this vision is of no importance. But all the greater importance attaches to the fact that many of the effects . . . can also be expected from a development that is clearly observable already. Progress itself may be mechanised."

For, he explains (p. 132), " innovation itself is being reduced to routine. Technological progress is increasingly becoming the business of teams of trained specialists who turn out what is required and make it work in predictable ways. . . . Thus economic progress tends to become depersonalised and automatised. Bureau and committee work tends to replace individual action " (p. 133). The part played by the capitalist entrepreneur ceases to be one of individual leadership, acting by virtue of personal force and personal responsibility for success, and this affects the entire bourgeois stratum which hitherto has been recruited and revitalised by absorbing the entrepreneurs and their families. The bourgeoisie " depends on the entrepreneur, and as a class lives and will die with him ".

" If capitalist evolution—' progress '—either ceases, or becomes completely automatic, the economic basis of the industrial bourgeoisie will be reduced eventually to wages such as are paid for current administrative work, excepting remnants of quasi-rents and monopoloid gains that may be expected to linger on for some time" (p. 134). There is, no doubt, a tendency for the individual free-lance innovator to be superseded by the big concern. The big concern can supply finance and organisation for new departures outside its original scope. And the opportunities may be discovered by individuals in its own employment.

But it is not clear that this portends a decay of capitalism. It may transform the character of the middle class, but Professor Schumpeter maintains that the middle class hitherto evolved by capitalism has lacked the qualities essential for political leadership, and has depended on the remnants of feudalism to provide a governing class. May not the new middle class be better qualified not only to conduct economic activity but to animate a political community? In any case, if it is an approximation to a socialist middle class, presumably a completely socialist middle class will manage no better.

His view of the future is shaped by his theory of profit. And his idea that without individual innovating enterprise profit will vanish or become unimportant is surely a profound miscalculation. Indeed, his theory is one more instance of the propensity of economists to explain away profit. Marx's theory of exploitation belongs to the fallacies of a past age. But the present age has its fallacies too. The profit-making which communism would suppress is not dependent on innovations or on monopolies. It arises, with all its anomalies and abuses, from the mechanism of competition, perfect or imperfect, and is the remuneration of selling power. It would persist therefore in the stationary state, or under conditions of depersonalised and automatised progress. Profit-making would still be the object of attack. In proportion as the profit-makers become fewer in numbers, they become more open to attack by the multitude, whether by revolutionary action or by the peaceful operation of democracy.

Between socialism and democracy, as conceived by Professor Schumpeter, there is no necessary connection, but there is no incompatibility (p. 284). He is sceptical of the classical doctrine of democracy, postulating a popular will (Chapter XXI). But his own definition of it, as a method "by which individuals acquire the power to decide [in the political field] by means of a competitive struggle for the people's vote", is surely not adequate. The competitive struggle, occurring when there is a cleavage of opinion, and parties offer alternative governments, is a frequent and lively development in democracies. But it often happens that there is no competition and no struggle; the preference of the electorate for a particular group of statesmen is recognised to be decisive, and oppositions are confined to occasional bodies of criticism.

Moreover the struggle for power, if it is to be democratic, must be one of persuasion. Actual democracies are imperfect, and persuasion is adulterated with intimidation or corruption, but intimidation and corruption are not part of the democratic method.

Professor Schumpeter's attitude to democracy as well as to socialism and capitalism is that of a tolerant but ironic critic. Whoever is carried away by great issues, it will not be he. But that is a very salutary approach. His book is full of wisdom, even though of a detached and fatalistic kind.

R. G. HAWTREY.

From Economic Theory to Policy. By E. RONALD WALKER. The University of Chicago Press. 1943. viii + 273 pp.

Professor Walker has written on a subject of great interest to the many economists who have recently enjoyed the privilege of helping to apply economic analysis to the stubborn questions of practical policy. Professor Walker has written an essay on *Economic Advising*. How should the contribution of the economic advisers be organised—in elaborate advisory councils or by the personal contacts of economic advisers scattered here and there among different government agencies? How should the economic adviser be trained? How should academic work on economics be reorganised so as to make it more relevant for practical economic policy? How should the scope of the economists' interest be extended with this same object in view?

Two extreme views are possible. On the one hand, are those who feel that contact with the real problems of government shows present-day economic analysis to be so restricted in its scope as to leave out of account considerations essential for the formulation of practical policies and to be founded on such assumptions as to make it theoretically satisfying but practically unrealistic. On the other hand, it may be argued, certain broad principles which are obvious to one who is trained in economics are not necessarily obvious to one who is not so trained, and that experience has proved that these principles cannot lightly be neglected.

Professor Walker is anything but an extremist. His book gains greatly from the sane and moderate way in which he states his point of view. But he inclines (a little too much perhaps) to the former of these two views. Consideration of some of the main contents of his book may serve to illustrate this point. Professor Walker inveighs in his chapter on "Theoretic Blight" against economists who analyse problems on unreal assumptions rather than concentrating on finding solutions to practical problems. But is he really sure that the cure of cancer will come from a cancer research institution rather than as the result of the fundamental research of a biologist who has no "practical" aim? Professor Walker suggests in his chapters on "The Problem of Behaviour" and "Beyond the Market" two fields in which the scope of the economist's interests, with or without the aid of other social scientists, should be extended: he must have better knowledge of the psychology of the motives which cause economic subjects to behave as they do, and he must formulate a theory of extra-market operations whereby (e.g., through lobbying for legislation favourable to their interests) economic subjects rig the market in their favour. No sensible person will quarrel with Professor Walker's positive suggestions ; but this should not be taken to imply that economic analysis as it exists at present has not an important contribution to make. It takes all sorts to make a world—even the world of economic advisers ; and one of the necessary sorts is the economist thoroughly versed in economic analysis as we now know it.

At the risk of appearing over-confident in his own profession, the economist may legitimately argue that recent experience of economic advising has proved the relevance of such analysis. If demand is increasing and prices are held down by rigid price control, what will be the next problems to prepare to meet ? Queues and shop shortages. This answer which comes instinctively to an economist does not come instinctively to those who are untrained in economic analysis. This may be a puerile example. More complicated and fundamental examples could be found (i) where choice is to be made of the most economic way of achieving a given end, or (ii) in the realm of problems involving total national income, expenditure, consumption, savings, etc. In the former an economist who is well versed in marginal analysis (even though some of his exercises in the past have been tinged with " theoretic blight "), and in the latter one who is well practised in the logical relationships between these global quantities, will have a valuable contribution to make.

Is the foregoing paragraph too self-confident ? I think not. Economic analysts have made their contribution, and have assured themselves once and for all that their subject is relevant. But this is not to detract from the enormous advantages which they, in turn, have gained from contact with real problems and with wise and able administrators and politicians ; nor have they failed to learn with Professor Walker that there are many ways in which their analysis must be expanded.

J. E. MEADE.

A Treatise on War Inflation. By WILLIAM FELLNER. University of California Press. 1942. 180 pp. $2.

This academic and rather difficult study examines the political and economic feasibility of avoiding inflation in wartime. I cannot feel that in either respect it adds much to existing knowledge.

Professor Fellner suggests in his Preface that readers interested in the general character of the problem should confine themselves to the first and last of his six chapters, omitting the " detailed factual analysis " of the remaining four. The reader adopting this course, however, will find that Chapter VI, except where it repeats Chapter I, is barely intelligible ; knowledge of the argument at least of Chapter V is indispensable to an appreciation of the compromises suggested.

Briefly summarised, the book's thesis is that War Finance is forced to be irrational because political agreement between different groups of the population is never sufficient to permit of an economically sound fiscal policy. A rational programme would involve : (*a*) borrowing from the banks only enough to match increases in aggregate output ; (*b*) borrowing from industry its undistributed profits and the proceeds of capital consumption ; (*c*) raising about 60 per cent. of the Budget by individual income taxes ; and (*d*) supplementing the last-named by relatively small corporation and excise taxes. As, however, these concrete proposals are in Chapter V, the general reader is left in the dark about what Professor Fellner would really do when he got down to brass tacks.

Being myself an unrepentant advocate of much heavier taxes in wartime, I entirely support his general aim. I am sure, however, that he under estimates the difficulties. He argues, for instance, that high marginal rates of income tax need not diminish incentive because they would fall principally on professional men whose future prospects depend largely on their present gross incomes (pp. 11, 115). But his specific proposals involve raising by income tax a sum equal to 30 per cent. or 35 per cent. of the National Income, and this means imposing a marginal rate of tax higher than 50 per cent. on all incomes exceeding $2,000 a year. Translated into terms of British incomes this group would include a very large proportion of piece-rate workers in the engineering and other war industries, and experience has shown only too clearly that the incidence of high marginal rates on the earnings of that class weakens immensely the incentive to increased effort—diminished in any event by the comparative ease with which unprecedentedly large incomes can be earned by the family efforts.

A similarly defective insight seems to me to impair the discussion of deferred savings. Professor Fellner regards as unreasonable Lord Keynes' argument that the accumulated savings can be beneficially distributed to maintain post-war incomes. He rejoins that a Government wishing to subsidise consumption in a post-war slump could do so whether deferred savings were available for release or not. But it is surely doubtful whether a National Dividend is yet practical politics at a time when a slump seriously threatens the income side of the Budget. I should like to think that this *was* a political possibility, but the very realism which the author elsewhere advocates suggests the wisdom of storing up commitments which can be met at such a time without challenging fiscal rectitude just because they do represent accumulated promises.

A short appendix illustrates American trends of price and output between 1939 and 1941 by statistics and charts. The latter are, however, rendered unnecessarily hard to interpret by being drawn to a logarithmic scale which is not marked on the ordinates for the range between 100 per cent. and 200 per cent. inside which most of each chart lies.

J. K. HORSEFIELD.

Economics in Uniform: Military Economy and Social Structure. By **Albert T. Lauterbach.**
Princeton University Press. 1943. London: Humphrey Milford, Oxford University
Press. xi + 282 pp. $3.00.

The prime intention of this book is, most fortunately, not to supply another treatise on war economics, nor is it an analysis of the wartime transformation of economic life in the strict terms of economic theory. It is rather a kind of political gestalt psychology of its subject, covering the whole field from Wehrwirtschaft to the questionable shape of things to come. If some of its statements are discussed by the reviewer in a critical spirit, this is not meant to detract from its value as a most readable description of political and economic forces in action.

The author does not resist the fashionable habit of digging into the historical roots of "the idea of military society in Germany", and he gives pride of place to two names: Carl von Clausewitz and Dr. Walther Rathenau. Clausewitz's *Vom Kriege* is certainly in its painstaking logic, its conciseness of style, its cool evaluation of the human element and with its truly scientific modesty a great book to which even disagreeing strategists and tacticians from all sides will always remain indebted. But if it is a question of finding roots it seems very doubtful if one has not to go back to a more general European denominator. This passionless treatment of a subject in which fundamental religious and moral problems are involved belongs to the attitude which the so-called age of the renaissance developed towards the theory of politics. Clausewitz's famous sentence which the author quotes (p. 58) that "War is not only a political act, but . . . a continuation of political intercourse . . . by other methods", is not specifically Prussian or German. No modern statesman has ever acted as if he had any doubts of its essential truth. In this, as in his insistence on the "absolute" character of modern wars, Clausewitz is unknowingly walking in the steps of Machiavelli who had clearly seen the close connection between politics and war. The decisive difference lies not so much in the beliefs of politicians of the various nations as to the function of war but at what point they regard the transition to "other methods" as necessary. And paradoxically enough it can easily be shown that the Germans, in spite of their great teacher Clausewitz, are more than other nations inclined to be carried away by the lust of war and to lose sight of its character as an instrument of politics.

If, nevertheless, Clausewitz is fully entitled to a niche in a gallery of ideological ancestry the author is seriously at fault in coupling Walther Rathenau with him. In this case even his facts are wrong, probably based on an English source which, however, he does not quote. After having rightly stated that at the outbreak of the 1914 war Rathenau tried to convince military leaders that raw material supplies must be centralised, he continues: "For a time he was ridiculed, for how could a businessman, and a Jew, teach Prussian generals the right way to conduct a war which, in any case, was to be won within a brief period? In the second year of the war, he was finally given charge of German raw material organisation, and he began working in the Prussian War Department with a staff of three, including typists" (p. 63). If the author had consulted the *Gesammelte Schriften*, which he is quoting on the same page, he would have found in Volume 5 a reprint of Rathenau's lecture from 1915 on "Kriegsrohstoffversorgung" in which he faithfully tells his own story: On August the 8th, 1914, he went to see Oberst Scheuch, Chief of the General War Department to whom he explained his concern about the raw material situation. When deeply worried, he reached, home in the evening, he found already a telegram from the Minister of War, von Falkenhayn, summoning him for an interview on the following day, a Sunday. The discussion lasted for several hours at the end of which Rathenau was asked to establish forthwith a Kriegs-Rohstoff-Abteilung: ". . . and thus I found myself, a few days later, installed in the Ministry of War" (*Ges. Schriften*, V, p. 28). The author's error of facts might not be regarded as a very serious one if it did not help to create or to sustain a Rathenau legend for which there is no real justification. Lauterbach says: "Rathenau was perhaps the greatest protagonist of neo-mercantilism" (p. 65), but actually Rathenau had been a protagonist of many and contradictory things, although perfectly convinced of his own arguments at a given moment. He was, however, neither an original thinker nor a creator of ideas and movements. He selected in the wardrobe of the spirit with an almost faultless sartorial sense for the opportunity; but these are the qualities of an actor and not of a statesman. He was by no means dishonest in a petty moral sense, but his was essentially a catalytic function and in the depth of his lonely heart he knew it. He himself does not claim to be the originator of the raw material policy. He gives full credit for this to Wichard von Moellendorf whom he immediately drafted into his new department. It is worth quoting the lines which von Moellendorf wrote to him on that very August the 8th, 1914: "Ich rate, das Kriegsministerium anzuregen, es moege den Status recherchieren und einen sparsamen Haushalt unter zentraler Gewalt eroeffnen". This sentence indeed reads as if copied from the Acta Borussica of the 18th Century.[1]

The author rightly refrains from giving any narrow definition of war economics. But his description of the theory and practice of German Wehrwirtschaft makes it quite clear that the object under review is certainly in uniform but that often there must be serious doubts whether it may still be called " economics ". Before outlining the aggressive totalitarianism of the Nazi-dominated European economy, whose prime object is swag to be gained by a mixture of military, political and economic coercion (p. 184), Lauterbach mentions the wave of largely indiscriminate technological " rationalisation " which swept over Germany on the flood of American credits after 1924. He might have drawn these facts somewhat closer together. For the German entrepreneur, in contrast to those of the western world, has a propensity to play with machines and buildings without strictly calculating the soundness of this form of " conspicuous " investment. If it does not pay in the ordinary economic sense there are always networks of cartels and tariffs to keep up prices and make the consumer bear the burden. This attitude, if compared with an over-anxious conservatism, has certainly some elements of merit in itself. But it also shows how much nearer German economy is to a state of affairs in which many of the traditional concepts of economic behaviour have lost their relevance (p. 186). The German war economy is in reality one side of a political revolution, what the Germans like to call " Aufbruch der Nation ".

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[1] Cf. Otto Goebel, *Deutsche Rohstoffwirtschaft im Weltkriege*, Stuttgart and New Haven, 1930 (Carnegie Series), p. 20; also Harry, Graf Kessler, *Walther Rathenau*, Berlin, 1928, p. 188. The final portrait of Rathenau with which the writer agrees has been given from personal knowledge and yet with malice toward none by Kurt Singer, *Staat und Wirtschaft seit dem Waffenstillstand*, Jena, 1924, p. 144ff. But even Kessler's biography, the book of a friend, is clear enough in its implications. In 1919 von Moellendorf joined hands with Wissell in a project of Planwirtschaft which Rathenau bitterly resented as being a caricature of his own ideas. Moellendorf collected his essays under the appropriate title of *Konservativer Sozialismus*, Hamburg, 1932.

The author, although avoiding sweeping judgments, is inclined to think that the western powers should have followed Germany much earlier and more completely in the military transformation of their economic structure.[1] He states with obvious misgivings : " The belief that a strong peace economy was not only a spur to international security but at the same time the best way of securing economic preparedness, was the rule " (p. 187). He even opines that the fall of France, which we should think was the consequence of some kind of political malaise, was caused by the " reliance upon latent resources, and the belief in the continuation of peacetime business until the declaration of war and even afterwards " (p. 187). Now, it is certainly true that, for instance, the exchange of goods between the United States and Japan in the pre-war years, with Japan importing exclusively strategic raw materials against the export of high grade consumers' goods, looks silly enough in the glaring light of later events (cf. p. 200). But to change this kind of " peaceful " international trade not only by degrees but fundamentally, would have meant to start the open war so much earlier, and this is less a question of economics than of political timing. But without going into this intricate problem the judgment even on purely economic grounds may still have to be reserved. A German writer on war economics, P. Wiel, conducted in 1937 a careful and objective investigation into the advantages and disadvantages of the time-lag in the armaments of totalitarian and democratic states. He showed how precarious Germany's position would be if the speculation on a short war should prove wrong and he did not overlook the mental strain which a long period of actual preparedness involves. (" Wirtschaft und militaerische Kriegfuehrung ", in *Deutscher Volkswirt*, 1937, p. 327f.) Lauterbach himself draws attention to the fact (see p. 78ff.) that several other writers on German Wehrwirtschaft were rather sceptical of its whole concept, and one may add that the organ of the German General Staff, *the Militaerische Rundschau*, repeatedly expressed the opinion that a careful roping in of the private economic motives would be preferable to their complete suppression.

Coming to his final chapter : " What after military economy ? ", the author gives a prudent warning to those who in laudable enthusiasm like to forget how dangerously in social science analysis and programme can become intermingled. The author shows the ambiguous character of the word " Planning " as a scientific term and would like to see it replaced by a completely different word, although he rightly does not dissociate himself from the justifiable hopes enshrined in it. And since, with a quotation from General Wavell's speeches, he touches on the problem why the unity of purpose in wartime cannot be kept alive in the peacetime society, I would wish to say a short word about this conundrum which seems to be a fruitful source of questions wrongly put.

The unity of purpose in war economics and political life in general during a war has many and great virtues indeed. But from the viewpoint of political science it must be regarded as a retrograde stage of social life whose real richness, in contrast to the animal societies, is formed by what modern philosophers have called the heterogeneity of purposes. We may recall Spencer's famous definition of evolution (First Principles, §145), but we prefer to go back to Immanuel Kant's *Idea of a universal history on a cosmopolitical plan*" (1784), of which Proposition the Fourth runs thus : " The means which nature employs to bring about the development of all the tendencies she has laid in man, is the antagonism of these tendencies in the social state—no farther, however, than to that point at which this antagonism becomes the cause of social arrangements founded in law. By antagonism of this kind I mean the *unsocial sociality* of man ; that is, a tendency to enter the social state combined with a perpetual resistance to that tendency which is continually threatening to dissolve it " (in Thomas de Quincey's translation, *Speculations, literary and philosophic*, London, 1861, p. 138). This sublime definition of a social equilibrium might serve as a warning to those who show a tendency to flee from an inhuman totalitarianism into a state where the immanent force of economic regulation may break irresistibly through the dams of democratic control.[2]

E. Rosenbaum.

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[1] For the *locus classicus* of this time-honoured political dilemma compare Pericles in *Thucydides*, *Poloponnesian War*, II., § 39.

[2] " Unity there should be, both of the family and of the State, but in some respects only. For there is a point at which a State may attain such a degree of unity as to be no longer a State, or at which, without actually ceasing to exist, it will become an inferior State, like harmony passing into unison, or rhythm which has been reduced to a single foot " (Aristotle, *Politics*, II, 5, edition Jowett, Oxford, 1885, p. 35).

The Institutional Theory of Economics. By RADHAKAMAL MUKERJEE. Macmillan & Co., Ltd. 1942. 357 pp. 10s. 6d.

Professor Mukerjee is dissatisfied with the abstract and "competitive" approach of orthodox economics, and seeks to replace it by a realistic study and analysis of the development and institution of particular economic systems or societies (not necessarily co-extensive with national barriers). By this means, he maintains, new and more realistic economic laws and generalisations will be obtained. His main thesis is that economists should analyse how economic factors function in particular environments, and types of environment—*i.e.* the working of economic groups and institutions—as contrasted with the analysis of the economic reactions of individuals under given but often abstract and unrealistic assumptions (which, in his opinion, results in logically correct, but unrealistic and therefore useless laws and theories). He admits both that the institutional method is still at an experimental stage, and that "abstract" economists have recently tended to introduce more realistic assumptions, but he claims that the former method has already resulted in the formulation of certain broad economic generalisations (for instance the "theory of social equilibrium in its successive levels", the "theory of optimum returns and optimum population", the "theory of economic and ultimate values" and the "theory of economic rights"), and apparently believes that the advance of abstract economics towards realism has been, and still is, too slow to yield fruitful results.

The advantage claimed for the institutional method is that interest would thereby be shifted from "the simple competitive postulate of economic individualism" to a realistic treatment of the incentives and other forces at work in the actual world, the latter being studied in the framework of the actual social and political organisation of the countries concerned, so that the economist would be in a position to visualise and evaluate the different aims of economic action, and the goals of different "plans" or economic policies, instead of being restricted to the purely economic aspect of the civilisations and peoples studied.

All economists agree that economic laws cannot work in a vacuum, and that in order to understand how economic forces work it is necessary to understand the natural, cultural and institutional background. But this is a very different thing from losing faith in the value of abstraction, or from holding that the study of institutions is—by itself—likely to result directly in the discovery of general economic laws. Professor Mukerjee's thesis, therefore, suffers from three main defects. In the first place he appears to misunderstand the economist's objectives and methods. He is battering at an open door when he says that an "adequate theory of human behaviour" cannot be formulated by considering individuals apart from the institutional setting. Economists do not aspire to the formulation of a theory of human behaviour. They accept human behaviour as part of the data on which they have to work, and make no attempt to "explain" it. The economic law that, other things being equal, a rise in price tends to stimulate supply, does not imply that every individual is primarily actuated by the economic motive, but merely that, in an unplanned society, the economic motive is, in fact, sufficiently strong and widespread to bring about the above-named result. In the second place it seems unfortunate to attack economics just when the attempt is being made—more and more successfully—to extend the hypotheses underlying abstract analysis from the highly simplified assumptions with which the classical economists worked to the more complicated and realistic basis which underlies (for instance) present-day studies of imperfect competition. Economists do, in fact, tend to take into consideration "the changing framework of law, public opinion and economic institutions . . ." (p. xiii), as advocated by Professor Mukerjee, and hence to "bridge the gulf between economic theory and policy". In the third place, Professor Mukerjee's method would cause economics to overlap seriously with sociology (which he admits should be the general meeting ground of the social sciences), and there seems no advantage in recommending economists to abandon their specialist studies and become sociologists! Moreover sociologists themselves rely on the work of specialists. Sociologists aim at utilising the results of the specialist studies of the various social sciences, in order to construct an integrated view of society as a whole.

Finally it must be suggested that although Professor Mukerjee makes interesting incidental points and comments in the course of his broad discussions of particular economic institutions and societies, and of their development, he fails to provide evidence that work of this type will result in the emergence of economic laws, in any accepted sense of that phrase, whilst his method of exposition—unlike that adopted in many of his very readable earlier works—is extremely verbose, often highly abstract, and anything but clear.

VERA ANSTEY.

Standardised Accountancy in Germany. By H. W. SINGER. *Occasional Papers*, V. N.I.E.S.R. Cambridge University Press. 1943. 68 pp. 6s.

This is the fifth of a series of *Occasional Papers* published for the National Institute of Economic and Social Research. In it Dr. Singer has performed the very useful service of bringing together in one small volume such material as is available in this country concerning the German experiment in introducing uniformity of accounting over almost the whole field of German industry. Unfortunately, the information is incomplete in many important respects, but in spite of the missing pieces there is sufficient of the jig-saw left to give us a rough impression of the largest experiment in uniform accounting so far made.

The decree which marked the birth of the new accounting system was issued in November, 1937, towards the end of the first Four-Year-Plan. It was addressed to the Reich Groups for Industry, Trade and Handicrafts, but not to Transport or Public Utilities, nor to Banking and Insurance, which had their own special regulations. The system was built upon the framework of the compulsory organisation of industry in groups. It was to become compulsory on 1st January, 1940, and by the middle of 1939 had been introduced by 24 out of the 33 groups.

On the accounting plane, the regulations provide, firstly, for a " Model Chart of Accounts ", which is uniform for all industrial firms and for all trading firms, and specifies no more than broad general headings of accounts; and, secondly, for a " Plan of Accounts ", which is uniform only within each group and contains more detailed sub-divisions of the model chart to suit the requirements of each industry concerned. Then on the costing plane, there were issued in 1939 certain " uniform costing principles "—a body of general principles applying to all industrial and commercial undertakings; these were to be supplemented later by " cost-accounting directives " to be issued by each trade group to deal with its own special problems within the framework of the general principles. But while uniformity in presentation of results was thus ensured, no steps were taken to ensure uniformity of method. Firms were left free to organise their bookkeeping, and to centralise or decentralise it, as seemed best to them. They were given the end, and left free to devise their own means of achieving it.

It will be seen that the Germans have drawn a sharp distinction between accounting and costing. Accounting is the recording of basic information; costing is the use of that information for specific purposes. If uniformity is desired, the first thing to make uniform is the accounting; once the uniform accounting system is running smoothly and yielding satisfactory data, uniformity of costs can be ensured simply by the issue of the necessary instructions as to which items of expense and income classification are or are not to be included for particular purposes. In their view, uniform accounting must precede the production of uniform costs.

Beyond that, there is little in this book that is new to the accountant who is well versed in the modern developments of cost accountancy in this country and in America. True, the German system includes an attempt to separate real costs of production from money costs, and to develop a sort of quantity accounting while still using money values as a means of expression. But the recipe (according to the information given) is incomplete, and experience of parallel attempts in this country makes one somewhat sceptical of the outcome of their experiment. The proof of the pudding, of course, is in the eating, but the war has prevented our tasting it.

One thing that emerges from a critical study of the German plan, as given in Dr. Singer's book, is the rigidity, and retarded development, of ideas, which seems inevitable in an attempt to impose a regimented uniformity on all accounting within a country. The idea of national uniformity was first mooted by Professor Schmalenbach, of Cologne, as long ago as 1928: and, as a matter of fact, the classifications of expense contained in the charts and plans now being imposed reflect the more advanced thought of approximately that period in this country. Since then, a lot of water has flowed under the bridge. Presumably accounting thought has not been stagnant in Germany either, and yet there is no trace in the German plan, to take one example, of any understanding of the significance of the distinction between fixed and variable costs—a significance which has gradually been permeating British thought and practice for the last ten or fifteen years. If uniformity is desirable—as it must be, if we are ever to have a common economic language—it is also desirable that it shall not be pushed too far, say beyond the limits of the different trade groups, so that independent thought can flourish in a variety of surroundings and a way can be left open for rivalry and the continuous evolution of ideas.

But what economists, accountants and others concerned in these matters are even more interested in than the technical processes necessary to establish uniformity, is the question whether the German plan really does what it set out to do. Not " what does the blueprint of the machine look like ? ", but " does the machine work properly when it is built ? ". We urgently need an answer to these questions, for uniformity of accounting may have an important part to play in our evolving economic system. As Mr. Henry Clay says in his preface : " Whenever by any means the test of productive efficiency imposed by competition in a free market is removed, a comparison of costs of different concerns becomes the only guide to an economic price, and such a comparison is impossible without standardised accounts ". It is, of course, not necessarily possible even then, because for the comparison to be effective there must also be reasonable uniformity in product and process. However, German experience might have taught us much. But on these matters, Dr. Singer is, in the main, silent—and necessarily so, for the curtain of war was rung down on the stage before the play had really started.

BASIL SMALLPEICE.

Creative Demobilisation. Vol. I by E. A. GUTKIND ; Vol. II Edited by E. A. GUTKIND. London : Kegan Paul. 1943. Pp. 331, 280. 21s. each volume.

" This book attempts to lay down *principles* of planning on a national scale. It is not concerned with the working out of details which belong rather to the sphere of regional and local planning. But these latter could not produce the desired result without the framework of a master plan." In this quotation we have the justification for Dr. Gutkind's collection of six hundred close pages. In the first seven chapters he himself outlines the main argument, which he then applies to agriculture and industry. There follows a sketch of the new arrangements in our settlements which such a *national* plan would require. After an inevitably compressed note on the demographic problem, he ends the first volume with a summary on "creative demobilisation", in which he outlines more concrete proposals] for the immediate future, and concludes with some admirable aspirations.

In the second volume various writers contribute short "case studies" on such subjects as the human geography of our various areas, on agriculture and industry in relation to a national plan, on the development of electricity nationally, and on the redistribution of towns and settlements. It must at the outset be recognised that such snippets cannot be expected to do justice to their authors' views. They may, however, be justified so far as they illustrate and illuminate the ideas in the first volume. Certainly, Professor Ashby and Professor Jones cannot touch their respective subjects without adorning them. But no one should go to these short essays to find a full statement of their theories. Also, in such a mere collection, one cannot expect to find any discussion of priorities as between the proposals of the different writers—and one does not, in fact, find it.

Clearly then the value of the work must stand or fall by its success in putting over some fundamentally new approach to the problem of settlement. And, in fact, this book will succeed in impressing on any reader who has the stamina to reach page 600 the importance of rising to a *national* view, and the rather revolutionary influence of such an ascent on all our ideas about town and country planning. In the process we also absorb some interesting ideas illustrating the position that "a self-contained community is an absurdity". These ideas are best developed in the chapter on Decentralisation and Dispersal. There, the description of zoning, such as is demanded by a national plan of living conditions, is excellently balanced, and is definitely attractive and convincing as an ideal. The picture of regions, framed and divided by main transport highways which are combined with fingers of park land; of the spaces so enclosed being then apportioned according to function into homogeneous areas for industry or residence or commerce, with elastic agricultural belts: this picture does suggest that we can and should create something which is neither country nor town, but which preserves the virtues of each and the vices of neither. Balanced settlements of various sizes will be combined in a balanced nation. And, in more detail, the concrete neighbourhood-unit is described in distinctly appealing characters.

We certainly should dream some dreams. But visions alone may produce the visionary; and a reviewer for an economic journal is especially bound to add some warnings. In the ideal world scarcity disappears; and in these pages economists will find no discussion of their specific problem. Only from Professors Ashby and Jones do we get shrewd comments on the vital issue of priorities. Certainly, Dr. Gutkind worked under limits: "We have promised that this book will not deal with politics". But if politics means the discussion of political and financial issues, and not merely party politics propaganda, then it is doubtful whether the promise should ever have been given.

It also follows from the deductive approach that there is a startling absence of foundation on the facts. What most economists wish to examine is how Poplar or Tyneside can be rebuilt; how the existing price and cost structures can be evolved towards better living. But of such evolutionary guidance the book is innocent—with the partial exception of Mr. Smailes' excellent study of "Ill-balanced Communities". Even such hopeful developments as the Trading Estates are not mentioned. Whereas the author is prepared to suggest the migration of whole industries, economists would feel more comfortable if some actual case were examined, based perhaps on new methods or power sources or raw materials.

A further result is that exclusive emphasis on rather theoretical machinery may inculcate a dangerous delusion. However willing the spirit, we are all flesh. And the merely ideal approach does suggest that all can be put right by machinery and re-arrangements alone. In our hearts we know that nothing can be done without self-control and self-sacrifice—that is, without economy and morality. But there is the risk that nobly intentioned books like this one will encourage us to "pass the buck"—to the government and the over-burdened civil servant: a form of laziness which has done more harm in the British community of late than any more positive moral weaknesses. The hard facts of our destiny are certainly obscured by such sweeping proposals for the better distribution of our population as are suggested in the chapter on the Demographic Problem. To such proposals the economist must oppose (as Professor Jones does) the dependence of the standards we have achieved on such hard facts as our power to export.

In sum, an attractive and suggestive, if rather laboured picture, very properly stressing the national aspect; but with even more reason than Cromwell, we should demand the warts.

A. L. MACFIE.

The Bills of Exchange Act, 1882. *With brief notes on analogous instruments and the Stamp Laws.* By MAURICE MEGRAH. 3rd ed. London (Pitman), 1942. xx + 203 pp.

The merit of this small textbook on the law of bills of exchange is that it provides the student with an elementary introduction into some of the legal principles governing the bankers' daily practice in relation to cheques and similar instruments. Much the most valuable part of the book is the concluding part which deals with cheques and analogous instruments, and which contains a succinct exposition of the Stamp Laws. The author is not quite so successful in his discussion of the general law of bills of exchange. From the point of view of English and American (not Continental) law the cheque is, of course, a species of the genus bill of exchange, and no one can hope to master the law of cheques who has not obtained a firm grasp on the law of bills. The author's description of the contents of the Bills of Exchange Act, 1882, suffers from a shortcoming all too frequently met with in legal textbooks: he embarks upon a course of pathology before he has given the student his elementary training in anatomy and physiology. Even the introductory chapters deal with such pathological phenomena as "non-issue" (where, incidentally, the author does not seem fully to appreciate the difference between the non-issue of a blank acceptance and that of a complete bill), fraud, theft, loss, and forgery. The ordinary incidents of negotiation and the mutual liabilities of the parties are only discussed in Chapter V. In his analysis of the meaning of "negotiation" the author fails to give the student a correct description of the difference between assignability and negotiability. Some of his remarks on assignment—especially in the introduction—are in need of revision. What he has to say on important subjects like accommodation bills, bills and cheques signed by directors and other officials of companies, and other similar matters, is of the scantiest, while acceptance supra protest, bills in a set, and lost bills are discussed at some length. There are a number of minor errors in the discussion of damages, and some unnecessary repetitions. However, the student who has mastered another textbook can use this small book with advantage, and derive profit from Mr. Megrah's explanation of some aspects of the law of cheques.

O. KAHN-FREUND.

Commodity Control: An Exposition of the Emergency Legislation Imposing Control of the Manufacture or Supply of Commodities other than Food. By J. BRAY FREEMAN and N. HOWARTH HIGNETT. [Reprinted from Butterworth's Emergency Legislation Service (Annotated). Butterworth & Co. 1943. 15s. 298 pp.

This imposing volume provides an admirable guide to and explanation of the emergency legislation affecting the manufacture or supply of no less than one hundred and eighty-one commodities (other than food) in Britain, and will prove of great value to the businessman, the lawyer, the economist and others interested in war-time production, trade, government control and administration.

The main object is to set forth, within reasonable compass, the provisions which operate to-day in coordinating and controlling national output, rather than to discuss the development of control, to apply credit or blame for the measures taken, or to assess the results. For instance, if one wants to know how and by what authorities the production, distribution and sale of coal is exercised; what orders have been issued referring to coal during the war and the scope of these orders; the powers of the Ministry of Fuel to divert coal consignments; or the effects of war legislation on the reorganisation that was being undertaken before the war, this volume immediately supplies a clear answer. On the other hand no attempt is made to deal with the effects on production, trade, etc., of this war-time legislation and administration.

The method of treatment is to "present a narrative exposition" of the emergency provisions, the full text of these provisions having been already made available in Butterworth's Legislative Service, to which references are made throughout. It is justly claimed that this method has two great advantages—first, that the effect of these provisions can be given in simpler and clearer language than is contained in the actual legislation; second, that it is thus possible to incorporate the effect of the many amending orders which so often succeed the original provisions. To keep the work up to date supplementary matter is to be published in cumulative supplements, to fit the pocket in the back of the cover.

Little more can be attempted here than to indicate briefly the contents and arrangement of the volume. It starts with tables of Statutes, Statutary Orders and Cases quoted, in each case giving full references. The "Introduction" then explains the object, scope and arrangement of the work. Then follow 13 parts (or chapters) dealing with the creation and function of war-time Ministries, the Emergency Powers (Defence) Acts, and with the provisions affecting employment, trade, production, prices, retailing and special industries. Part 12 discusses the effect of control upon pre-existing contracts, and Part 13 deals with offences, penalties and proceedings.

The rest of the work consists of eight Appendices, dealing with such matters as the concentration of production, the control of building operations and the local price regulation committees.

In conclusion it need only be said that the authors have succeeded admirably in their declared object. The arrangement is clear and logical and the annotations enable the layman to follow with facility the usually complicated and often confusing language of the actual legislation and orders.

VERA ANSTEY.

Report on the Constitutional Problem of India. Part I, *The Indian Problem, 1833–1935.* Part II, *Indian Politics, 1936–1942.* Part III, *The Future of India.* By R. COUPLAND. Oxford University Press. Part I, 1942, 158 pp. 6s.; Part II, 1943. 338 pp. 7s. 6d.; Part III, 1943. 205 pp. 6s. 6d.

This admirable survey is the result of a study involving two visits to India (during the second of which Prof. Coupland was attached as an unofficial adviser to the Cripps Mission) and consultations with Indian leaders, as well as with innumerable officials and public authorities, carried out under the auspices of Nuffield College, Oxford. It presents the best and most impartial analysis that has yet appeared of the Indian problem, and suggests a novel solution of the present deadlock. Even those who disagree with many of the author's conclusions, detect a bias (as, no doubt, many will do, although they may disagree as to the nature and direction of the bias !), and consider his proposals inadequate or wrong-headed, will agree that he has tackled the subject in a scientific spirit, has attempted to take an objective view of a most difficult, complicated and controversial problem, and has made his proposals in all sincerity, with the sole object of helping to solve what has hitherto proved an intractable problem.

Part I is mainly historical. Part II presents a most enlightening study of the working of the Indian Constitution since 1935, of the characters and policies of the chief political and communal groups (especially of Congress and the Moslem League) and of the Indian States. The impact of the war and the causes of the present political deadlock are also discussed.

Part III is most interesting of all, and as it begins with summaries of Parts I and II, it can be read independently by those who have not time or inclination to read the whole triology. It may, however, be hoped that this procedure will not be generally adopted, as those not already conversant with the Indian scene would thereby miss a most lucid record and analysis, whilst even those who are well acquainted with Indian affairs will discover many illuminating comments and explanations in the first two volumes. It is, however, with Part III that I shall be mainly concerned in this review.

The main conclusions upon which Professor Coupland's proposals are based are that the present deadlock can only be ended by compromise (" each side must admit the strength of the other side's case ") ; that an appropriate and acceptable compromise cannot be found along traditional lines ; that previous attempts to solve the problem have been frustrated, on the one hand by too rigid and blind adherence to the British constitutional model, and, on the other hand, by the attitude and methods of Congress since 1935 ; and hence that a new start should be made, involving partly the incorporation of constitutional principles and methods characteristic of certain democratic constitutions, though not of the British constitution, and partly the adoption of novel constitutional expedients. " Most of the discussion on the question in India," he says, " seems hitherto to have been unduly narrowed by preconceived ideas."

It is impossible to discuss here the details of Professor Coupland's most ingenious and carefully thought-out proposals for a solution, but its main features may be briefly indicated. He starts from the assumption that if the present deadlock persists, partition must follow, as the Moslems cannot be forced to bow down to a Congress hegemony, and indeed the Draft Declaration of 1942 specifically concedes the right of Provinces and States to contract out of any Federation or Union that may be constituted in the future. He argues, however, that partition would be politically undesirable, and economically and financially disastrous. His proposals are therefore designed to maintain at least a degree of unity for India as a whole, without, however, depriving the Moslems of their claim for a " national home " and the substance of self-determination. These apparently contradictory aims can, he thinks, be attained along regional lines ; that is, by combining the various Provinces and States into (say) four regions, based on geographical, economic and communal features. Luckily geographical (specifically " river-basin "), economic and communal factors can well be combined for this purpose. Two of these regions would have Moslem predominance, and two Hindu predominance—although all would necessarily contain important minority elements. " That would mean an even communal balance at a Centre based on the Regions." Whilst retaining considerable Provincial Autonomy (the helpful suggestion that Provincial Constitutions need not be identical may be noted in passing) substantial powers would be given to the Regional Authorities which would have to be created. The four Regional Authorities would themselves appoint representatives or agents to form some kind of Central Union for India as a whole, which would be endowed with limited and specific functions (e.g. foreign affairs, defence, tariffs and currency), but would not possess supervisory or residual powers. In support of the proposed compromise the author says " the Moslems must recognise—as surely indeed they already do—that, if they could agree with the Hindus as to how it could be done, it would be better for them both to preserve the unity of India than to destroy it. And the Hindus must recognise—and that may prove more difficult—the Moslem claim to nationhood and national self-determination."

All details would be subject to discussion and modification, and the author himself suggests a number of possible alternatives. Indeed the elasticity of the scheme is one of its most valuable features. The author's main object is to provide a basis for negotiations to end the deadlock, so that the various communities may work out their constitutional future by common agreement.

Hardly less important than the proposals for regional grouping are the suggestions for the separation of the Provincial and Regional Executives from the Legislatures. It is proposed that the Provisional Governments should be Statutory Coalition Governments, not responsible to the legislature from day to day, but appointed for a given period. As Professor Coupland says, " What the minorities need is not so much protection against the abuse of power as participation in its exercise." On these points he suggests that the character and operation of the Swiss Constitution seem worth examination.

There are, of course, many other proposals affecting both the points already mentioned and others which I have no space to discuss. The scheme undoubtedly deserves serious consideration by Indians of all parties and communities, and one can only hope that they may prove willing to give this consideration. Even if the author's particular preferences and the details of his proposals do not prove acceptable, there is a chance that some valuable and workable compromise might emerge from negotiations opened on the basis of the regional principle.

A work of this description, on a highly controversial subject, is obviously open to many criticisms on points of detail. But with regard to really important matters, I wish to put forward one criticism only. This refers to the author's explanation of the failure of Indian constitutional development since 1919. He accepts Lord Linlithgow's attitude that failure has been due to the rapidity of the constitutional advance. To my mind the reverse is true. If the substance of the Cripps proposals had been put forward in 1930, in place of the egregious Simon Report, does anyone doubt that it would have been joyfully accepted and that an agreed Constitution would by now have been instituted and at work?

VERA ANSTEY.

India. Guide to Current Official Statistics. Vol. I. Production and Prices. Prepared under Instructions from the Economic Adviser by S. SUBRAMANIAN. 1943. Delhi. Rs. 4.

This Guide provides a most admirable account of what statistical information is available in Indian official publications, of the degree of completeness and reliability of the information, and of the sources from which and methods by which the information is compiled. The present volume deals with primary production, industrial production, and prices. A second volume is promised, to deal with trade, transport and communications, finance, and social welfare. The procedure adopted is, in the first place (pp. 1–53) to give a detailed account of all the principal recurrent central government publications, and to cite as reference publications both occasional central government publications of an all-India character (such as the reports of Commissions and Committees, the Tariff Board Reports and the Marketing Survey Reports) and the publications of the Provincial Governments. In the second place there is a lengthy Index (pp. 55–137) divided into two parts: the first part gives the principal publications in alphabetical order, with reference to the relevant explanatory pages of the Guide; the second part gives a subject index, with references to the Guide and to the relevant official publications.

Even a hasty glance through this Guide provides ample evidence in support of the generally accepted view that a necessary prerequisite of economic reconstruction in India is the collection of more ample and more reliable facts and figures. It also shows how little has been done to remedy the statistical deficiencies so clearly pointed out in the Bowley-Robertson Report on the Scheme for an Economic Census of India, as long ago as 1934.

Here it must suffice to refer to four obvious deficiencies. Firstly—as the Guide points out—the official estimates of agricultural output are not only inaccurate, but have a definite "downward bias" which renders them most unreliable, if not positively misleading, when used (for instance) as a basis for calculating the available food-supply of India, or for estimates as to whether or not the food-supply has kept pace with population growth. Secondly, the information in practically every subject is extremely incomplete for the Indian States. Thirdly, there is hardly any statistical information with regard to cottage industries, which still employ by far the greater part of India's industrial population. In fact the hand-loom industry is the only cottage industry for which All-India estimates of production have ever been attempted. Fourthly, the prices collected are so unrepresentative that it has been decided to cease publication of the quinquennial "Index Number of Indian Prices".

Let us hope that the publication of this Guide will not only help interested readers to obtain the information that is available, but will also stimulate the provision of more and more adequate statistical information.

VERA ANSTEY.

Trade Regulations and Commercial Policy of the United Kingdom. By the Research Staff of the National Institute of Economic and Social Research. Cambridge University Press. 1943. 269 pp. 15s.

The great merits of this admirably concise and clear exposition of the methods of trade regulation adopted in the United Kingdom, between the two wars, are its logical and systematic arrangement (which make it an excellent reference book), the attention paid to important but often neglected aspects of commercial policy (such as the careful explanation of the machinery of the tariff and of the commercial agreements, and the account given of the practice which affords incidental protection) and—above all—the masterly manner in which the main economic problems of particular industries and other types of production are depicted in a few short paragraphs. Indeed this slight and slender volume contains more "meat" than many others of twice its length.

The Introduction surveys briefly the change in and development of commercial policy since before the last war, indicating clearly the critical periods and legislation, and the chief types of protection and other assistance afforded to various types of producers. Part I then deals in more detail with the development of tariff protection between 1913 and 1938. Appendices to this Part provide a summary of the tariff in operation in 1939; a list of tariff exemptions and preferences; an account of the drawbacks given on imports which are subsequently re-exported; and a table of net receipts from customs duties in 1913–14 and from 1930–31 to 1937-8 inclusive.

Part II deals with non-tariff protection, including the protection of agriculture and non-tariff assistance to various industries. Relevant statistics and other information are given in appendices. Part III deals with commercial agreements, and appendices provide relevant trade statistics, together with lists of (*a*) the chief commercial agreements in force between 1931 and 1938; and (*b*) the multilateral treaties and agreements in force on Jan. 1, 1939. Part IV deals with incidental protection. The appendices to this part provide not only lists of prohibitions and restrictions on importation, but also a summary of the restrictions on trade with particular countries, and of the prohibitions and restrictions on exports.

This study was originally undertaken as (the British) part of a more general, international survey of modern commercial policies. It is to be hoped that the other national studies will eventually be published, and will prove as valuable as the volume at present under review.

VERA ANSTEY.

The Economist, 1843–1943. A Centenary Volume. Oxford University Press. 1943. 178 pp. 7s. 6d.

The Economist, 1843–1943. [A reprint of the articles contained in the Centenary Number of *The Economist*, published on September 4th, 1943, with some other matter.] 79 pp. 2s.

The war has in more than one way curtailed benefits which economists and historians might in different circumstances have hoped to derive from the centenary of the *Economist*. The records and the library of the journal were destroyed in its 98th year as a result of enemy action, and paper shortage has further reduced the amount of information that could be supplied. Yet the "modest memorial", offered in place of the tome the occasion would have justified, much surpasses in content as well as in appearance what we were entitled to expect in the given circumstances. Indeed, as a book to read rather than as a work of reference, the slim volume is preferable and lacks only the documentation of which events have deprived us.

The volume begins with two surveys of the *Economist's* career from the pens of the present Editor and Deputy Editor, cloaked in the traditional anonymity, which the *Economist* almost alone preserves, and whose value is strongly and convincingly defended. These surveys are, as one would expect, pleasantly and lucidly written and only a little marred by some over-emphasis on the continuity of the political outlook, which almost suggests that sub-consciously the authors are a little doubtful about it. This is followed by an altogether admirable chapter on "*The Economist* as a Source for the Historian" by Sir John Clapham. It is a mine of information and one is rather surprised to notice that in terms of the number of words into which it is compressed it is the shortest but one of the essays in the volume. In the more general "Political Retrospect" by Professor E. L. Woodward that follows the *Economist* is lost a little out of sight. A sympathetic account of Walter Bagehot by Mr. F. W. Hirst comes next, but one regrets that no space has been found for a similar full portrait of the founder of the *Economist*, James Wilson, who, though not as big a figure as Bagehot, deserves to be better known than he is. Even as an economic theorist Wilson is by no means negligible, and while there is a separate chapter by W. W. Rostow on "Bagehot and the Trade Cycle", there is hardly a reference to Wilson's interesting views on the same subject.

Of the four other essays not yet mentioned Mr. Graham Hutton's instructive study on "*The Economist* and Foreign Affairs" suffers perhaps more than any of the others from the impossibility of extensive quotations, which would have demonstrated even more convincingly the astounding solidity of the judgment of N. W. Senior, which is the main point that emerges. Mr. W. T. C. King's chapter on "The Money Market" is for the professional economist nearly the most interesting of the volume, while Mr. H. Parkinson's section on "The Stock Exchange" and the Industry and Trade Editor's on "Records and Statistics" are best described as, what they were clearly intended to be, eminently useful.

The second item listed at the head of this review, a pamphlet issued unfortunately under the same title as the centenary volume, is noted here mainly to warn librarians. "It contains all the special centenary articles which appeared [in the *Economist*] on September 4th, [1943], dealing with the history, policy and present views of the paper, together with the speeches made at a centenary luncheon on September 2nd," and can (or perhaps could) be obtained from the publishers of the *Economist*.

F. A. v. HAYEK.

Economics. By F. BENHAM. Sir Isaac Pitman & Sons. Third Edition. 1943. 534 pp. 7s. 6d.

The fact that this book has reached its third edition in five years is sufficient proof of its popularity, and it is well deserved, for Dr. Benham has succeeded in writing the best general introductory text of Economics for the student that exists to-day.

The present edition differs from its predecessors principally in the addition of two new chapters on the Theory of Costs and on War Economics, while the chapter on Interest has been completely rewritten.

Of the new material, Chapter XVI on the Theory of Costs is excellent and adds materially to the usefulness of the book for students. The final chapter on War Economics attempts to cover a very wide field, both in theory and application, and is not so successful. The most interesting changes, however, are to be found in Chapter XVIII on Interest. In dealing with this most hotly debated and controversial of problems in economic theory, Dr. Benham gives first an account of the classical doctrine, and then goes on to give a very balanced summary of the newer (and at first sight conflicting) views put forward by Lord Keynes and his followers. The author does not profess to set before his readers a completely satisfactory reconciliation of the two doctrines (no economist as yet can claim to have done this), but his analysis comes nearer to achieving this than many more ambitious attempts; and the chapter as a whole is to be commended, not merely to students, as an illuminating introduction to the problem of Interest.

C. W. GUILLEBAUD.

Can We Afford It? By OSCAR R. HOBSON. P. S. King & Staples, Ltd. 1943. 172 pp. 5s. net.

It is welcome news that the reception of his earlier small volume on *Does Money Matter?* has encouraged Mr. Hobson to follow it up with another, somewhat longer collection, of the masterly short articles in which he comments from day to day on economic events. In his combination of open-mindedness towards new ideas with an essential sanity of outlook and the capacity to elucidate difficult matters in the terse sentences of a few brief paragraphs, Mr. Hobson has to-day probably no equal among financial journalists.

Apart from articles reprinted from the city columns of the *News Chronicle* the present volume also contains three articles reprinted from *The Spectator* and two which have not appeared before. They are grouped in seven sections headed "Saving and Spending", "Profit Motive, Competition, Monopoly", "Banking and Credit", "Industrial Reconstruction", "Trade and the Future", "International Money" and "Freedom from Want".

Early in the book there occurs the observation that "neither the paradoxes of yesterday nor the commonplaces of to-day necessarily represent the last word in wisdom. Sometimes we plunge from one extreme to the other." It is the constant awareness of this in which Mr. Hobson shows his essential sanity. Though immediately concerned with the problems of the day, he is acutely alive to the current of ideas. Whenever one of the characteristic ideas of the moment has been given by somebody a particularly clear expression, Mr. Hobson seizes upon it and a fair representation as often as not is followed by an explanation why the argument leaves him entirely unconvinced. Whether the reader will everywhere agree with Mr. Hobson or not (the reviewer finds himself in almost complete agreement), I believe he will find the book as useful a guide to straight thinking on the typical problems of economic policy as any other that is available.

F. A. v. HAYEK.

SHORTER NOTICES

The Economic Pattern of World Population. By J. B. CONDLIFFE. Planning Pamphlets No. 18, National Planning Association, Washington, D.C. 1943. 54 pp. 25c.

This is a useful summary of a large field which includes trends and distribution of population, growth of cities and other matters. The merit of the treatment is that the author has not overweighted the text with facts and figures; he has selected his data and used them to illustrate the main problems and the outstanding aspects of the world population story as it unfolds itself at the present stage of history. The pamphlet should be of real value to those who want an outline of demography to-day.

Will the War Make Us Poorer? By MICHAEL YOUNG and HENRY N. BUNBURY. Oxford Pamphlets on Home Affairs No. 5. Oxford University Press. 1943. 6d.

This short pamphlet contains a straightforward and lucid popular exposition of the methods of war finance and their effects on our post-war conditions. Though it recognises that in the recoil from orthodox tenets of finance "recently the pendulum has been swinging too far", it errs probably still a little in the same direction. But the views expounded in it are those now held by the majority of economists in this country, though the conclusions are perhaps slightly more optimistic and the neglect of the effect of the loss of most of our income from foreign investments on the future terms of trade mars the otherwise fairly balanced picture. The discussion of the role of saving in normal times is entirely Keynesian.

The Economic Reconstruction of Lithuania after 1918. By ANICETAS SIMUTIS. New York: Morningside Heights. Columbia University Press. London: Humphrey Milford. 1942. xiii + 148 pp. 10s. 0d.

This is an account of the economic structure of Lithuania, the largest in population and the second largest in area of the three Baltic republics. Lithuania is mainly an agricultural country, over three quarters of her population being engaged in agricultural occupations. The author accordingly devotes a large part of his book to a discussion of agricultural structure, co-operation and similar topics. Other chapters deal with foreign trade, industry and finance. Within the limits of this short book, there is no room for very detailed treatment, and it is possible that the necessary statistical material for a discussion of income distribution, national income and output and allied subjects which are not considered in the text does not exist. Nevertheless, the book is welcome as an introduction to the study of the economic structure of a region, about which little is known in this country.

The Distribution of Population and the Location of Industry on Merseyside. By WILFRED SMITH. The University Press of Liverpool. 1942. 133 pp. 5s. 0d.

This is a very detailed survey of population movements and industrial location in the Merseyside conurbation. In the first part the author deals with migration and natural population increase, showing that in Merseyside, as in other industrial centres, a shift from the centre to the periphery has taken place. In the second part the industries of the region are surveyed one by one. The whole work is done in great detail, using published and unpublished data. The conclusions will be of interest and importance to those charged with making good the war damage suffered by that area.

Merchandise Availability in Utah. By D. E. FAVILLE, D. M. JONES, and R. B. SONNE. Stanford University Press. 1942. xvi. + 229 pp. 20s.

This research study was planned to answer the questions " To what extent can country people buy wanted items of merchandise as readily as people living in town ? " and " To what extent must country people pay more for these items ? " The enquiry (which involved 2,156 consumer interviews, and covered 24 per cent. of the total rural population, and 21 per cent. of the total population of the four " control cities ", in the chosen areas) was admirably planned, along lines recommended by the best authorities on market research. The results are set forth with the utmost clarity in text, tables and diagrams, and the publishers are to be congratulated on the extremely pleasant appearance of the volume. Unfortunately (or is it fortunately ?) the conclusions are precisely what was to be expected on *a priori* grounds : i.e., that the country dwellers were at a disadvantage as regards both choice and price of articles, and that the higher prices charged—in particular for drugs, groceries and clothing—in rural areas were especially felt by the low-income classes and the farmers.

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A Liquidity Preference Theory of Market Prices

By K. E. Boulding

The ultimate " causes of price "—to use a Classical term—lie deeply embedded in the psychology and techniques of mankind and his environment, and are as manifold as the sands of the sea. All economic analysis is an attempt to classify these manifold causes, to sort them out into categories of discourse that our limited minds can handle, and so to perceive the unity of structural relationship which both unites and separates the manifoldness. Our concepts of " demand " and " supply " are such broad categories. In whatever sense they are used, they are not ultimate determinants of anything, but they are convenient channels through which we can classify and describe the effects of the multitude of determinants of the system of economic magnitudes. These ultimate determinants are, on the one hand, the patterns of choice between alternatives of all individuals, and on the other, the pattern of technical limitation of resources such as labour time and raw materials and the transformation functions of these resources into commodities and services. It is possible to show, algebraically, how these billions of determinants operate to create and to change the structure of economic quantities (prices, volumes, etc.). Such algebraic demonstration, after the manner of the Lausanne school, though logically necessary, is not practically valuable in the solution of particular problems.[1] For this task we need to be able to divide the multitude of causes into a few broad workable categories, of which the Marshallian demand and supply analysis is an admirable example. It is not the purpose of this paper to overthrow the Marshallian methods, or to question their limited validity. Rather is it to suggest a new dichotomy of forces in the special case of market price in a competitive market, not into " demand " and " supply ", but into " price determining factors " and " quantity determining factors ". For many purposes it would seem that this new dichotomy is much more useful than the old, and leads to results which could not have been attained with the demand and supply apparatus.

It should be noted carefully that the situation discussed in this paper is that of the determination of market price in a perfectly competitive market on a single " day ", the " day ", of course, being as short as we wish to make it and being defined by the condition that the market is cleared and that all transactions which can be accomplished under existing conditions have been accomplished. This itself, of course, is a fiction, though a useful and necessary one: our mind finds it difficult to grasp the slippery and continuous processes of the actual world without first " fixing " each position in our mind before proceeding to the next. The simplest approach to dynamic economics is through a succession of static pictures, just as the representation of movement on the screen is obtained through a rapid succession of stationary projections. In the " market day " therefore we make the following assumptions: (i) the quantity of exchangeables (goods and money) possessed by all the people in the market does not change during the course of the " day ". All that happens is exchange, in the simplest and most literal sense of the world—i.e., a *rearrangement of ownership* of the goods and money in the market. At the end of the day some people have more money, some less; some have more commodity, some less, but the increases must be exactly balanced by the declines as the total quantities possessed by all marketers have not changed. (ii) We will assume that the price that clears the market is discovered immediately and prevails all through the " day ", and that all transactions made during the " day " are made at that price. This assumption does not quite correspond to reality: nevertheless if the " day " is sufficiently short no serious errors are likely to be introduced by it, and it enables us to sidestep a number of highly difficult problems of secondary importance concerning the effect of transactions on the market.[1]

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[1] The brilliant work of Professor Leontief (*The Structure of American Economy, 1919—1929*) might seem at first sight to disprove this assertion. But even Professor Leontief has to simplify his system of simultaneous equations radically in order to obtain any results.

The situation envisaged above must be distinguished sharply from the analysis of normal price by means of long-run demand and supply curves. In the normal price analysis we are considering an equilibrium of *flows* of commodity on and off the market. The demand curve in this case shows the average rate of consumption, and the supply curve the average rate of production, that would prevail at each price. It is dangerously easy to carry over habits of thought from the long-run analysis into the analysis of the market, yet the two are very distinct, and it must constantly be borne in mind in the following analysis that the quantities refer to *stocks*, both of commodity and money, that are shifted around in ownership, and do not refer to *flows* of production, consumption, or income.

We will begin the analysis by considering the situation of a single marketer in a one-commodity market, possessing a quantity of money m_1 and a quantity of commodity a_1. His willingness to buy or sell can be described by his individual market demand-supply curve or function, showing what quantities of the commodity he will buy or sell at each price. This will normally be a continuous function: at high prices he will sell, at low prices he will buy, and at some intermediate price, which may be called the " null price " he will neither buy nor sell, being satisfied with the quantity in his possession. It is convenient to adopt the convention that purchases (because they add to the stocks of the purchaser) are positive in sign, and that sales are negative. We can then express the individual market demand-supply function by a single equation,

$$q_1 = f_1(p) \tag{1}$$

A negative value of q_1 here represents an offer to sell, and a positive value an offer to buy.

In the market there will be a number of individuals, 1 to N, each having his own demand-supply function, $q_1 = f_1(p)$, $q_2 = f_2(p) \ldots\ldots q_n = f_n(p)$. Knowing these functions we can immediately derive the market demand and market supply schedules by adding, for each value of p, the positive q's to get the total quantity demanded, Q_d, and the negative q's to get the total quantity supplied, Q_s. Thus we obtain the demand function, $Q_d = F_d(p)$ and the supply function $Q_s = F_s(p)$, and the condition that the price must equate the quantity demanded and the quantity offered—i.e., $Q_d = Q_s$, gives us a third equation from which we can now derive the three unknowns, Q_d, Q_s, and p. This is the usual demand and supply market analysis.

There is, however, another way to approach the problem. If we add, algebraically, all the individual q's at each price we get a figure $Q_e = (Q_d - Q_s)$ which represents the excess demand or excess supply, according as it happens to be positive or negative. Thus by simple addition we derive from the individual demand-supply functions, an excess demand-supply function, $Q_e = F_e(p) = \Sigma f(p)$. Then at the equilibrium price we know that $Q_e = O$, i.e.,

$$\Sigma f(p) = O \tag{2}$$

What has been done here is to separate out that element in the market situation which is responsible purely for the determination of price. Graphically, the price is given by the point at which the excess demand-supply curve (which I have elsewhere called the " total market curve "[2]) cuts the vertical (price) axis. The " height " of the excess demand-supply curve is in some sense an " average " of the heights of all the individual demand-supply curves from which it is derived. That is to say, the market price will be some kind of weighted average (the weight, of course, depending on the type of individual demand-supply function) of the " null prices " of the various marketers.[3]

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[1] Marshall, we may note, avoided the same set of problems in his discussion of the " corn market " by assuming that the marginal utility of money remained constant.

[2] Boulding, *Economic Analysis*, p. 73. See also Hicks, *Value and Capital*, p. 63.

[3] It is easy to show that when the individual demand-supply functions are linear the market price is the weighted average of the null prices of all the marketers, weighted by the slopes of each individual demand-supply curve. Thus let the individual demand-supply functions be $p_1 = b_1 q_1 + c_1$, $p_2 = b_2 q_2 + c_2, \ldots\ldots p_n = b_n q_n + c_n$. Then $c_1, c_2, \ldots c_n$ are the null prices of the various marketers. The equilibrium price p is given by equation (2), i.e.,

$$\frac{p}{b_1} - \frac{c_1}{b_1} + \frac{p}{b_2} - \frac{c_2}{b_2} + \ldots\ldots + \frac{p}{b_n} - \frac{c_n}{b_n} = 0$$

$$\text{i.e., } p = \frac{\dfrac{c_1}{b_1} + \dfrac{c_2}{b_2} + \ldots\ldots + \dfrac{c_n}{b_n}}{\dfrac{1}{b_1} + \dfrac{1}{b_2} + \ldots\ldots + \dfrac{1}{b_n}}$$

The " null price " of an individual's demand-supply curve is a measure of his willingness to buy, or what is the same thing, his unwillingness to sell. Any increase in his willingness to buy raises his whole demand-supply schedule, and with it raises his null price. A rise in the market price therefore can only result from a general net increase in the willingness to buy the commodity in question. It is this, and no other factor, that determines price.

It is possible also to separate out those elements in the total market situation which determine the quantity exchanged. If all the demand-supply curves of the individual marketers passed through the same point on the price axis—i.e., if all the marketers had the same null price—the demand and supply curves would intersect on the price axis and there would be no exchanges on the market at all. There might be a market price quoted, but as this would of necessity be equal to the null prices of all the marketers, nothing would be bought or sold at that price. It is only because the different marketers have different degrees of willingness to buy or sell that transactions can take place at all. This property of the market we may call its " divergence ", and it is this, and this alone, which determines the quantity exchanged. The greater the divergence of the individual demand-supply curves—i.e., the greater the spread of the null prices of the various marketers about the market price, the greater will be the quantity exchanged.[1]

We have now effected our new dichotomy of the forces in the market, dividing them not into " supply " and " demand ", but into " price determining " and " quantity determining " factors. The descriptive advantages of such a division are obvious, and it is interesting to compare the description of various changes under our analysis and under the old supply and demand analysis. Suppose, for instance, that we have a " pure " price-raising movement, through a uniform increase in the height of *all* the individual demand-supply curves. That is, there is a uniform increase in the willingness to buy, or what is the same thing, a uniform decrease in the willingness to sell. This is reflected merely in the price, the quantity exchanged remaining as before. In our analysis it is represented simply by a rise in the total market demand-supply curve, which now cuts the price axis at a higher price than before. In the demand and supply analysis this same movement would be represented by a rise in the market demand curve coupled with a fall in the market supply curve, the new point of intersection being at a higher price, but at the same quantity as before. These movements in the demand and supply curves, however, are not independent; they are both the result of a single force, the increase in the willingness to buy or unwillingness to sell expressed by the rise in the individual demand-supply curves. Indeed, a situation is hardly conceivable in which a change in market demand does not go hand in hand with a change in market supply. It is this interdependence of market demand and supply curves that makes them rather unsuitable for market analysis.

Similarly, when we have a " pure " change in market divergence, resulting in a change in the quantity bought without change in the price, the result is a change in *both* supply and demand curves, proceeding from the same cause. Suppose that the divergence of the various individual null prices becomes wider, without any change in the market price. The quantity exchanged will increase. The market demand and supply curves will both move to the right, intersecting at a point representing a larger quantity, but the same price as before. The movement of both demand and supply curves thus follows from exactly the same cause: viz., the increase in market

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[1] Where the individual demand-supply functions are linear an algebraic formula for the quantity exchanged can be derived as follows, using the notation of footnote 3, p. 56. Let $c_1, c_2 \dots . c_k$ be the null prices of all those marketers whose null prices are greater than the market price, p. These are the buyers. The amount that buyer K will buy at the price p is $\frac{c - p}{b}$, and the total amount bought is

$$Qd = \frac{c_1 - p}{b_1} + \frac{c_2 - p}{b_2} + \dots . . + \frac{c_k - p}{b_k}$$

Similarly the marketers whose null prices are greater than p, c_{k+1}, $c_{+2} \dots . . c_n$, are the sellers, and the quantity sold, Q_s, is given by:

$$Qs = \frac{c_{k+1} - p}{b_{k+1}} + \frac{c_{k+2} - p}{b_{k+2}} + \dots . . + \frac{c_n - p}{b_n}$$

Q_s, of course is equal to Qd. The quantity exchanged, therefore, is equal to the weighted sum of either the positive or the negative deviations of the null prices from the market price weighted according to the reciprocal of the slope of the individual demand-supply curve. The greater these deviations—i.e., the greater the " divergence " in the market, the greater the quantity exchanged. If all the null prices were the same, then we should have $c_1 = c_2 = \dots . = c_k = \dots . = c_n = p$, and both Qd and Q_s would be zero.

divergence. It is evident that the division of the market situation into " demand " on the one hand, and " supply " on the other, does not correspond to any very significant distinction within the structure of the market. The reason for this is clear: in a competitive market " buyers " and " sellers " are not distinct groups of people, separate from each other. Any marketer may be a buyer at one price and a seller at a higher price: every rise in prices induces some who were previously in the buyer's camp to cross over into the seller's, and every fall in prices has the reverse effect. Hence the demand and supply curves cannot be independent: they represent only a momentary division of the market, and will invariably move together except in the unlikely case of exactly counterbalancing changes in eagerness to buy and in market divergence. This is not the case, it should be noticed, in the case of the long-run demand and supply analysis: the producers and consumers of a single commodity are usually different groups of people, with only a little overlapping. In this case it is reasonable to assume that demand and supply curves are independent of each other and of the price, and hence they have much more individual meaning and validity.

We can now take yet another step in the analysis, and actually define an equation for the individual demand-supply curve on a simple and plausible assumption. From this we shall go on to derive a simple yet revolutionary formula which gives us the market price itself. The assumption is that each marketer wishes to hold a certain proportion of his total capital resources in the form of money, which we will call the *preferred liquidity ratio*, r, or his " liquidity preference ", and that this proportion is independent of the absolute level of the price of the commodity. As we shall see, the preferred liquidity ratio is by no means independent of anticipated changes in prices, but there seems to be no reason to suppose that it would be affected by the actual level of absolute prices. This assumption alone is sufficient to give us an equation for the individual demand-supply curve and a simple formula for market price.

Let an individual marketer possess a quantity of money m_1, a quantity of commodity a_1, and suppose that his preferred liquidity ratio is r_1. Let q_1 be the quantity of commodity that he exchanges in the " day ": q_1 if positive will represent a purchase, if negative, a sale. Let the price be p_1. Then we will assume to begin with that he possesses only commodity A and money, and that no other commodities enter into the market, or, what is perhaps more realistic, that his other possessions and transactions do not affect his transactions in commodity A. Then after he has completed his transactions the total amount of commodity he possesses is $a_1 + q_1$, and the total amount of money is $m_1 - p_1q_1$. The total value of commodity possessed by him is $p_1a_1 + p_1q_1$. The total value of his holdings of money and commodity combined is therefore $(p_1a_1 + p_1q_1) + (m_1 - p_1q_1) = p_1a_1 + m_1$. It will be noticed that the total value of his holdings is not affected by the exchanges he makes, as what he receives is always equal in value to what he gives up. After his exchanges have been completed the ratio of the money he now possesses to the value of his total holdings should equal his preferred liquidity ratio, r_1, for presumably the *object* of his transactions was to rearrange the form of his possessions into the desired liquidity ratio. We have, therefore,

$$r_1 = \frac{m_1 - p_1q_1}{m_1 + p_1a_1} \tag{3a}$$

This equation can also be written:

$$p_1 = \frac{m_1(1 - r_1)}{r_1a_1 + q_1} \tag{3b}$$

or

$$q_1 = \frac{m_1(1 - r_1)}{p_1} - r_1a_1 \tag{3c}$$

This is the equation of the individual's demand-supply curve, for it shows what quantity of the commodity he will buy or sell at each price in order to achieve his preferred liquidity ratio. It is a rectangular hyperbola, asymptotic to the quantity axis at $q = \infty$, and to the line $q_1 = -r_1a_1$ at $p_1 = \infty$. This expresses the fact that an individual will never sell a greater proportion of the commodity that he possesses than his liquidity ratio, and will only sell all the commodity he possesses if he wants to have all his holdings in the form of money (i.e., if $r = 1$). The asymmetry between buying and selling is interesting: no matter how high the price rises the amount an individual is willing to sell cannot go beyond a certain point. There is no limit, however, to the increase in the amount that an individual is willing to buy as the price falls, for with each fall in price the purchasing power of his money increases.

It is now easy to obtain a formula for the market price, knowing the quantities of money and of commodity possessed by each marketer, and knowing the preferred liquidity ratio of each. Denoting the quantities associated with the various marketers by suffices 1, 2, ... n, and applying equation 2 we have immediately the market price, p, given by equation (4):

$$p = \frac{m_1(1-r_1) + m_2(1-r_2) + \dots + m_n(1-r_n)}{r_1a_1 + r_2a_2 + \dots + r_na_n} \tag{4}$$

This is a most instructive equation. It shows at once that an increase in the quantity of money, or a decrease in the quantity of commodity, possessed by any marketer, or a decrease in the preferred liquidity ratio of any marketer, will raise the price. It shows also that the effect on the price produced by a change in the quantity of money or of commodity possessed by any marketer depends on his preferred liquidity ratio. A change in the stock of commodity will have a larger effect on price if it is felt by those marketers with the higher liquidity preference. A change in the stock of money will have a larger effect on price if it is felt by those with a lower liquidity preference. Thus if new money gets into the hands of " hoarders " with high liquidity preference it will have less effect on price than if it gets into the hands of individuals with small liquidity preference (spenders). The equation also shows two interesting extreme cases: that if the preferred liquidity ratio is zero—i.e., if people do not wish to hold any money—the price will be infinite; if the preferred liquidity ratio is 1, so that people wish to hold all their resources in the form of money and none in the form of commodities, then the price will fall to zero. It also shows that when liquidity preference is high, a change in the stocks of commodity produces a greater effect on price than a change in the stocks of money. On the other hand, when liquidity preference is low, changes in the stocks of money produce more effect than changes in stocks of commodities.

If the preferred liquidity ratio of all individuals is the same and equal to r, equation (4) reduces to the very simple form

$$p = \frac{M(1-r)}{Ar} \tag{5}$$

where M is the total stock of money in the market and A is the total stock of commodity. Even if the preferred liquidity ratios of the various individuals differ, equation (5) is still highly significant: r then means the " average " or " market " liquidity ratio. It is the proportion of the value of liquid property to the value of all property which the market as a whole feels is most desirable. The market liquidity ratio, r, is not a simple average of the individual ratios, however, but a complex weighted average.[1]

The important equation (5) can easily be derived directly, without reference back to individual preferences. The total value of goods and money owned by marketers is $pA + M$. If r is the preferred liquidity ratio for the market as a whole, therefore, we have

$$r = \frac{M}{pA + M},$$

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[1] We have, combining equations (4) and (5)

$$\frac{M(1-r)}{Ar} = \frac{m_1(1-r_1) + m_2(1-r_2) + \dots + m_n(1-r_n)}{a_1r_1 + a_2r_2 + \dots + a_nr_n}$$

$$\text{whence } r = \frac{M\Sigma a_ir_i}{AM + M\Sigma a_ir_i - A\Sigma r_im_i} \tag{6}$$

If money and commodity were equally distributed among all the marketers this equation would reduce to the simple average of the individual r's—i.e., $r = \frac{\Sigma r_i}{n}$. Where there is not this equal division it should be noticed that the market liquidity ratio, r, could change somewhat even if the individual preferred ratios, r_1, r_2, etc., did not change, through a change in the a's and m's. It is not, therefore a wholly satisfactory measure of liquidity preference, but the same difficulty is encountered in any weighted average. Yet another simplification can be made: if it is assumed that the ratio of the amount of commodity to the amount of money possessed by each marketer is the same—i.e. $\frac{a_1}{m_1} = \frac{a_2}{m_2} = \dots = \frac{a_n}{m_3} = K$, then equation 6 reduced to $r = \frac{\Sigma a_ir_i}{A}$, i.e., the market ratio is the weighted average of the individual ratio, weighted by the amount of commodity possessed by each marketer. In such a case a decline in the amount of commodity possessed by a marketer with a liquidity preference, above the average, would tend to lower the market ratio even if the individual ratios did not change. Such an interpretation of the market ratio, however, is not unreasonable, and the assumption on which it is based is not likely to be far from the truth.

which when transposed immediately gives equation (5). This equation should be compared with Fisher's equation of exchange, to which it bears some resemblance. The liquidity preference concept and the concept of velocity of circulation are at bottom different ways of expressing the same phenomenon. It would be quite possible to apply equation (5) to the general price level. In that case p would represent the price level, M the total quantity of money, A the total quantity of commodities and other valuta not including money, and r would be the general liquidity preference ratio. An increase in liquidity preference is the same thing as a decline in the velocity of circulation—i.e., it represents an increased desire for money and therefore a decreased willingness to spend it. The ratio $\frac{(1-r)}{r}$ may therefore be called the " velocity ratio ", v: it rises and falls, along with the velocity of circulation of money, as r falls and rises. The differences between equation (5)—which may perhaps be called the " equation of price determination " and Fisher's equation of exchange are, however, highly significant. Equation (5) interprets prices in terms of *stocks* of commodities and of money, coupled with a preference factor. Fisher's equation describes simply an identity of *flows*: of money on the one hand and of the value of goods on the other. Although useful in many ways, it fails to give any causal explanations of the determination of price, for price and the quantity exchanged are always determined together; hence an equation which includes the volume of transactions is useless as an explanation of prices, for the volume of transactions is one of the unknowns which has to be determined along with price. Equation (5) has the advantage that all its components, except price, are historically determined or are given as data: the stock of money, the stock of goods, and the preferred liquidity ratios are objective facts at any moment of time, and may truly be described as the only short-run determinants of market price. There still remains the question " what determines the stocks and the preferences ": that, however, is a long-run problem of normal price.

From the stocks and preferences data we can also derive a formula which gives the quantity exchanged, or the volume of transactions in the " day ". The null price of marketer I, c_1, is found by putting $q_1 = O$ in equation 3b, whence

$$c_1 = \frac{m_1(1-r_1)}{r_1 a_1} \tag{7}$$

Substituting the value of $m_1(1-r_1)$ from equation 7 into equation (3c) we have:

$$q_1 = \frac{r_1 a_1}{p}(c_1 - p_1)$$

If the subscripts 1 to k represent the buyers in the market—i.e., those individuals whose null prices are greater than the market price, then the total volume of transactions, Q, is given by:

$$Q = \Sigma_1^k q_i = \Sigma_1^k \frac{r_i a_i}{p}(c_1 - p) \tag{8}$$

The volume of transactions is again seen to depend on the divergence of the null prices of the various marketers from the market price.

Substituting the value of p from equation 4 in equation 3c, summing $\Sigma_1^k q_1$ and simplifying, we arrive at the general formula for the quantity exchanged,

$$Q = \frac{M_d \Sigma_{k+i}^n r_i a_i - M_s \Sigma_i^k r_i a_i + (\Sigma_i^k r_i a_i)(\Sigma_{k+i}^n r_i m_i) - (\Sigma_{k+i}^n r_i a_i)(\Sigma_i^k r_i m_i)}{M - \Sigma_i^n m_i r_i} \tag{9}$$

If the liquidity preference of each marketer was equal to r, the formula simplifies to:

$$Q = \frac{r(A_s M_d - A_d M_s)}{M} \tag{10}$$

where A_d and A_s respectively signify the amounts of commodity possessed by buyers and by sellers, and M_d and M_s represent the quantity of money possessed by buyers and sellers. Although this formula is suggestive, it is not as useful as might at first sight appear, for as we have seen there is no clear distinction in the market between buyers and sellers. Nevertheless the formula suggests roughly that a rise in the quantity of commodity, or a fall in the quantity of money held by the " sellers " side of the market will tend to raise the volume of transactions, while a rise in the quantity of commodity or a fall in the quantity of money held by the " buyers " side of the market will have the opposite effect. It also suggests that a rise in liquidity preference, or a fall in the total quantity of money held in the market will have a general tendency to increase the volume of transactions, though this result is not necessary.

The analysis can now be extended without much difficulty to include any number of commodities. Suppose that there are Z commodities, $A, B, C, \ldots . Z$. Let the total value of all these commodities, plus the money held by the marketers be v. By " commodity " we here mean anything that has monetary value: it therefore includes securities and other valuta. We can now express the " commodity preference " for each commodity as the preferred ratio of the value of the commodity to the total value of all things, including money, held in the market. Thus if p_a is the price of commodity A, and A is the amount held by the marketers, the " commodity preference ratio " for commodity A, r_a, is given by the equation:

$$r_a = \frac{Ap_a}{v} \tag{11}$$

If M is the total quantity of money held in the market, and r_m is the liquidity preference ratio, we have:

$$r_m = \frac{M}{v} \tag{12}$$

Eliminating V between these two equations we have:

$$p_a = \frac{Mr_a}{Ar} \tag{13}$$

Similarly for the other commodities we have:

$$p_b = \frac{Mr_b}{Br},\ p_c = \frac{Mr_c}{Cr} \ldots . p_z = \frac{Mr_z}{Zr} \tag{13}$$

It follows that an increase in the quantity of money, or a decrease in liquidity preference, will raise *all* prices. An increase in the quantity of any one commodity will lower the price of that commodity; if the preferences of the market do not change, however, an increase in the quantity of one commodity will not change the prices of any other commodity.[1] This will only be true where the commodity is neither a substitute for nor a complement of any other commodity: i.e., where $\frac{\delta r_b}{\delta a} = \frac{\delta r_c}{\delta a}, = \ldots . = \frac{\delta r_z}{\delta a} = 0$. If the commodity A has a substitute, B, then an increased quantity of A in the hands of the marketers will cause a decrease in the preference for B: i.e., $\frac{\delta r_b}{\delta a}$ is negative. If the commodity A has a *complement*, C, then an increased quantity of A will cause an *increase* in the preference for C—i.e., $\frac{\delta r_c}{\delta a}$ is positive. In such a case an increase in the quantity of A will cause a decrease in the price of the substitute, B, and an increase in the price of the complement, C, according to equations (13). We see therefore that this method provides an easy solution for the problem of relatedness in demand.

It must be observed that there cannot be a change in one preference ratio without there being an equal compensating change in the sum of all other preference ratios, for the sum of all the preference ratios must be equal to unity: i.e.,

$$r_a + r_b + \ldots . + r_z + r_m = 1 \tag{14}$$

It follows that a change in price which is due to a change in *preference*, as opposed to a change in the quantities of goods or money, must be accompanied by opposite changes in other prices, unless it is counterbalanced by an equal and opposite change in liquidity preference or in quantities. Thus if the *preference* of the market for commodity A increases, without there being any change in the preference for money or in the quantities held, there must be not only a rise in the price of A but also a fall in the price of other commodities.

The mathematically inclined reader will not find it difficult to derive the general formulæ relating the price of each commodity to the individual preference ratios of each marketer for each commodity and for money. A general formula for the quantities exchanged can also be developed for the many-commodity case. As these formulæ involve complex and unresolvable determinants, however, they are of no practical value that I can discover, and are omitted.

The implications of the above analysis for economic theory are, I believe, profound. It is, in the first place, a powerful instrument for the unification of many parts of our existing theoretical structure which hitherto have been rather unrelated. It unifies, for instance, the theory of general prices and the theory of particular prices. As we have seen, if P is the general price level, W the total stock of all valuta, excluding money, R_w is the general commodity preference ratio and R_m the liquidity or money preference ratio, we have a formula for the general price level exactly analogous to equations (13) and (5):

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[1] It will, of course, lower the price level in so far as its own price is lowered.

$$P = \frac{MR_w}{WR_m} = \frac{M(1 - R_m)}{WR_m} \tag{15}$$

This price-level formula has an important contribution to make to the understanding of the crisis of late capitalism in which we seem to be living. The most striking feature of the past twenty years has been the strength of the deflationary forces in the western world. Equation (15) gives an important clue to this mystery. We see immediately that the total *value* of the stock of goods (PW) is equal to the quantity of money, M, multiplied by the " preference factor ", R_w R_m. It follows that if the quantity of money and the preference factor are constant, the total value of the stock of goods cannot change, for every increase in the quantity of goods will result in a proportionate decline in their price. In such a case investment, in the financial sense, is absolutely impossible, for by investment we mean the *increase* through time of the total value of goods. (By " goods ", of course, we mean all physical capital.) Investment is only possible if either the quantity of money increases or liquidity preference declines, no matter how rapid the accumulation of physical capital. The rate of investment therefore depends, paradoxically enough, directly on the monetary situation, and only indirectly on the rate of accumulation of goods. It follows immediately that if there is no change in the preference ratios the rate of investment is equal to the rate of growth of the monetary stock. In the absence of a growth of the monetary stock or a fall in liquidity preference the accumulation of physical capital must inevitably result in a deflationary movement of prices.

The commodity preference ratio is likely to fall and the liquidity preference ratio to rise on the expectation of falling prices, for when prices are falling the purchasing power of stocks of money is continually increasing while the money value of a given stock of commodities is declining. This in itself is sufficient to account for the self-justifying nature of price anticipations: if people expect a fall in prices, R_m will rise and R_w will fall and P will fall in the absence of counterbalancing changes, whereas if people expect a rise in prices R_m will fall and R_w will rise and a rise in prices will ensue.

There is no reason to suppose also that commodity preference will decline and liquidity preference increase as the total stock of goods increases. There is no point in piling up stocks for ever, and in the course of accumulation the time must come when further accumulation becomes less and less desirable. This will be reflected in a declining rate of profit, which will make the holding of goods less attractive relative to the holding of money. When accumulation has proceeded to the point where stocks of most physical goods are large there will be a deflationary force operating due to the decline in commodity preference. This will be reinforced by the expectation of declining prices ; hence even if money stocks keep pace with stocks of goods, there will still be a powerful deflationary force operating. In such a period a rapid increase in money stocks will be necessary to keep the price level constant. The equilibrium of the price system, however, will be difficult to maintain because of the expectational factor.

The present analysis also enables us to see the so-called " liquidity preference " theory of interest as merely a special case of equation (13). The true money rate of interest to be expected from a security is determined at any moment by the price of that security. The higher the price of the security, the lower the rate of interest assuming that the future payments accruing to the owner of the security do not change, There is no such thing, strictly, as a " market rate of interest "—it is not the rate of interest that is determined in the market, but the price of securities—i.e., of expected future payments-series. The rate of interest itself is not a price, like the price of wheat: it does not have the dimensions of price. It is merely a certain mathematical property of a series of expected payments and their present price. The price of securities is not determined by the rate of interest; the expected rate of interest (and all rates of interest are expected) is determined by the security's present price. The price of a security is determined by the same factors that determine the price of any commodity—the quantity of money in the market, the quantity of the security in the market, the liquidity preference ratio and the security preference ratio (the proportion of total resources which the market wishes to hold in the form of the particular security). Thus if S is the quantity of the security held, R_s the security preference ratio, the price of the security is given by equation (13): $P_s=\frac{MR_s}{SR_m}$. We see therefore that the price of a security will be increased, and the rate of interest will therefore be lowered, by an increase in the quantity of money, by a decrease in the quantity of the security, by an increase in security preference or by a decrease in liquidity preference.

A formal explanation of the difference in apparent yields of various classes and terms of securities can be given in terms of the " security preference " concept. Where a security or other item of property has certain desirable qualities apart from its monetary return—such as, for instance voting privileges, or a high degree of salability, or a certain prestige value, its " security preference " will be high, its price high and the rate of return correspondingly low.

The extension of the principles of this paper to the theory of the firm, to the labour market, and to the theory of monopoly and imperfect competition must wait for another paper. Instead of treating the firm primarily as a profit and loss account, as is done in the usual marginal analysis, it can be treated as a balance sheet, and its decisions described in terms of their effects on the balance sheet rather than on the income account. Thus a purchase of anything, whether it be raw materials, equipment, or labour, involves an asset transfer—a reduction in liquid assets and an increase in illiquid assets. Similarly a sale involves a reduction in illiquid assets and a gain in liquid assets. The willingness of firms to buy and sell can therefore be explained partly in terms of their preferred liquidity ratio. This aspect of a firm's behaviour is strictly analogous to the type of analysis we have employed in this paper. There is, of course, an additional complication that firms buy and sell not merely to change the form of their assets, but to increase their net worth. This is the sole assumption behind the refined marginal analysis. Thus it is assumed that a firm will extend its purchases of each factor of production until the discounted marginal productivity of the factor is equal to its marginal cost. The discounted marginal productivity, however, is what the purchase of the factor adds to the illiquid assets of the firm; its marginal cost is what the purchase of the factor subtracts from the liquid assets. We can rephrase the marginal productivity condition, therefore, and say that a firm will extend its purchases of any factor as long as there is a net addition to its assets. It will be seen immediately that the marginal productivity analysis assumes that the firm is quite indifferent as to the *form* of its assets—i.e., has an infinite number of preferred liquidity ratios. This assumption is very far from the truth, and changes in the liquidity preference of firms, as well as the composition of their assets, may have profound effects on their demand for factors of production. This is a factor that has been largely neglected in the theory of the firm, although it is implicit in much recent monetary analysis. The effects of an increase in liquid balances on the demand for labour and on the level of employment can hardly be explained without some reference to the liquidity preference factors in the demand for input.

This " asset-transfer " type of analysis provides, I believe, the most useful stepping-stone to the analysis of dynamic problems. Not only does it provide us with an " instantaneous " picture of price determination which can then serve as a basis for the " moving picture " technique of describing dynamic changes, but it also gives us a parameter which can reflect all the forces operating from the side of the future: viz., the preference ratios. Not only, therefore, have we separated out of the chaos of causes the price-determining and the quantity-determining factors; in the price-determining factors themselves we have distinguished between those that operate from the results of the past and those that operate from the expectations of the future. The quantity of money and of goods are given to us as a result of past events. The preference ratios are in part determined by future expectations. It would be difficult to devise a more suitable dichotomy for the analysis of the well-nigh inconceivable fluxes of reality.

Finance Capitalism ?

By L. M. Lachmann

It is easy to see that the extension of economic research into the realm of dynamics will necessitate much closer co-operation between economic theorists and historians than has existed so far. Not only have historians to provide the empirical material with which to test our dynamic models. But, as it is impossible to conceive of dynamic processes as taking place against the quasi-stationary background of neo-classical economics, we have to provide, first of all, a proper setting for our models, a World in Motion of which each dynamic process forms an element and from which it derives its impulse, its direction, and, hence, its significance. This World in Motion provides the substance of, if it is not identical with, the historical process itself.

There are, however, obstacles to this closer co-operation which cannot be removed merely by repeated expressions of goodwill. Theorists, for instance, might profitably abstain from flaunting " general " theories which are readily seen to rest on assumptions which are—and often enough are meant to be—mere transcriptions of the conditions of economic activity during some particular period of time, usually the theorist's own lifetime. Economic historians, on the other hand, are often confronted with a peculiar dilemma. In order to interpret the facts they have collected they need a theory, but the kind of theory best suited to their needs, viz. a theory with a time-dimension, theorists have thus far been unable to provide for them. Are we to blame them if then they evolve theories of their own, schemes of economic development " by stages ", models at the crudity and inadequacy of which the sophisticated may smile, but for which they have no substitute to offer ? Or are we entitled to scoff if in their distress they address themselves to the art-historians, their seniors, to borrow their central concept, the Styles of Art, which they then adapt as " economic systems " ?

We believe that in promoting a more perfect harmony between theory and history the theorist will be most useful if his approach is made along the following line: He must, first of all, attempt to understand what problems the historians are trying to solve in evolving their various schemes and models. He should then examine the facts which figure prominently in these models as to whether they are facts relevant, or the facts most relevant, to the problems to be solved. If he is not satisfied with the relevance of the facts stressed by the historians, he must send them out for new facts. If he is satisfied with the relevance of the facts gathered by the historians, but finds their model incoherent or otherwise inadequate, he must out of the same facts construct a better model.

In the present paper we shall follow this procedure in examining certain views on the economic character of our time. In a terminology which seems to have found fairly wide acceptance this is described as an epoch of Monopoly Capitalism and Finance Capitalism. The latter epithet appears to merit closer attention than it has hitherto received. A whole literature has grown up in recent years around the problem of monopoly, its theoretical depth has recently been probed by Professor Schumpeter, and the impressive number of volumes embodying the result of the labours of the T.N.E.C. investigation testifies to the interest it has found in political circles. But Finance Capitalism has, on the whole, been neglected since Dr. Hilferding first described the phenomenon in 1910,[1] and although a good deal of economic literature on current events implicitly reflects a theory of it, this theoretical position is hardly ever precisely stated or even recognised as such.

What is Finance Capitalism ? Dr. Hilferding defines " Finance capital " as " capital controlled by banks and employed by industrialists " and describes it thus:

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[1] Rudolf Hilferding: *Das Finanzkapital*, Vienna, 1910.

" A steadily increasing proportion of capital in industry does not belong to the industrialists who employ it. They obtain the use of it only through the medium of the banks, which in relation to them represent the owners of the capital. On the other hand, the bank is forced to keep an increasing share of its funds engaged in industry. Thus the bank is increasingly being transformed into an industrial capitalist. This bank capital, i.e. capital in money form which is thus really transformed into industrial capital, I call ' finance capital '."[1] Lenin, writing in 1916, thus enlarged on the subject: " When carrying the current accounts of a few capitalists, the banks, as it were, transact a purely technical and exclusively auxiliary operation. When however, these operations grow to enormous dimensions we find that a handful of monopolists control all the operations, both commercial and industrial, of the whole capitalist society. They can, by means of their banking connections, by running current accounts and transacting other financial operations, first *ascertain exactly* the position of the various capitalists, then *control* them, influence them by restricting or enlarging, facilitating or hindering their credits, and finaly they can *entirely determine* their fate, determine their income, deprive them of capital, or, on the other hand, permit them to increase their capital rapidly and to enormous dimensions."[2]

If it were true that Finance Capitalism, so defined, is one of the outstanding characteristics of our time, this proposition would be significant in at least three respects. It would, first of all, be a genuine contribution to the understanding of our time and its problems. Secondly, it would probably throw light on some problems which have resulted from the theoretical study of contemporary phenomena, like the investment problem or that of the various forms of monopoly, and thus be of help to the theorist. Thirdly, it might possibly establish a test case for the feasibility of " shortcuts ", for the possibility of gaining knowledge directly from the inspection of historical material without the lengthy and awkward roundabout journey of theoretical analysis. For these reasons, if for no others, the problem of Finance Capitalism would seem to merit a more detailed study.

CLARIFICATION

If we are to follow the procedure just outlined, our first task is to clarify the meaning of " Finance Capitalism " by finding out what problem the introduction of this concept was meant to solve. The only way in which we can attribute meaning to a notion purporting to describe an economic phenomenon is by translating this notion into the vocabulary of economic theory. For economic theory provides us with a coherent frame of reference which permits us to see economic phenomena as the—intentional or unintentional—outcome of the pursuit of plans, and it is by reducing " Action " to " Plan "—and by this activity of the mind alone—that we " understand " human action and can attribute meaning to it. Throughout this paper we shall assume that by Finance Capitalism is meant a type of economic development which is characterised by the shifting of the entrepreneurial function into the hands of " financiers ", investment bankers, i.e. intermediaries of the capital market, specialists in directing capital flows. We now have to show, first, that this is what our authors " really meant ", that this is the sense in which their utterances have to be construed ; and, secondly, that the thesis that Finance Capitalism in this sense is an outstanding feature of our age is a meaningful proposition, that whether true or not, and whatever the range of phenomena covered, it " makes sense ".

Now, as to the first, it is easily seen that if Dr. Hilferding says that " the bank is being transformed into an industrial capitalist ", what he means is a shifting of the function of industrial enterprise to the bankers, for the mere legal creditor-debtor relationship is economically irrelevant. If he defines finance capital as " capital controlled by banks ", the word " control ", we submit, must denote the exercise of the active, planning and decision-making function which characterises the entrepreneur, and which in a dynamic economy determines the direction in which it moves; otherwise the location of this control would economically be a matter of indifference. Similarly, if Lenin says that the financiers " first *ascertain exactly* the position of the various capitalists, then *control* them, influence them . . . and finally they can *entirely determine* their fate"[1], the process he is describing is, we suggest, economically a shifting of the entrepreheurial function.

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[1] Op cit., p. 283.

[2] V. I. Lenin : " Imperialism, the highest stage of Capitalism." *Collected Works*, Vol. XIX, p. 110. (Italics in original.)

In order to accomplish the second part of the task of this section, viz. to show that the contention that Finance Capitalism is an outstanding feature of our age is a meaningful proposition, we must give a brief outline of the position which "the entrepreneur" occupies in modern economics. As long as the family firm and the private partnership were the preponderant forms of business organisation the location of the entrepreneurial function was no problem. There were occasionally "sleeping" partners, it is true, but as a rule it was not difficult to identify those who were wide awake. But with the growth of joint-stock enterprise the question "Who is the entrepreneur?" became a pertinent one. It is true that the official legal theory which underlies Company Law continues to ascribe this rôle to the shareholders,[2] but a whole literature from Walther Rathenau to Mr. Burnham has shown this to be a fiction, even to those naïve minds who had not yet gathered as much from the financial press. That to ascribe the entrepreneurial function to the shareholders is absurd, is obvious enough once we have adopted the definition of enterprise as planning and decision-making activity in a World of Change. Unfortunately this is far from being generally accepted, no doubt because so much of modern economics is still under the shadow of the static pattern of the classical school. In particular, it has to be remembered that the theory of entrepreneurship has only gradually detached itself from the theory of profit, and to understand this problem we must therefore make a brief excursion into the history of economic thought.

The method by which the classical economists demonstrated the unity of the economic system was to design a theoretical pattern of symmetrical shape in which to each type of income—Wage, Rent, Profit—there corresponded a "factor of production"—Labour, Land, Capital. When under the influence of J. B. Say and his school profit came to be more clearly distinguished from interest, it became necessary, for the sake of symmetry, to invent a conceptual receptacle for this type of income; and thus the entrepreneur made his entry into economic theory. As the theory of profit developed it became evident that profit—unlike other forms of income—cannot exist in a stationary state, at least under competition. Then J. B. Clark showed that profit is a concomitant of economic change, and it seemed natural to conclude, as Professor Schumpeter did, that the entrepreneur is the man who engineers change. But unfortunately the main current of discussion turned in a different direction.

Professor Knight, discussing J. B. Clark, pointed out that "the fatal criticism of this procedure of taking changes in conditions as the explanation and cause of profit is that it overlooks the fundamental question of the difference between a change that is foreseen a reasonable time in advance and one that is unforeseen".[3] While this distinction and its corollary, the expectational nature of all action concerned with the future, have become the starting-point of most modern dynamic theory, it is to be regretted that it apparently has led Professor Knight to neglect another distinction, equally important, between man-made change and exogenous change, between change that is "made" and change that "occurs".[4] His argument, for all its incisive brilliance, runs almost exclusively in terms of knowledge and judgment; purpose and plan have no place in it. His is a world in which exogenous changes "just happen" to men who, passively, react to them with varying degrees of foresight, the higher degrees of it giving rise to profit; it lacks the active element of change planned by man. For his own purpose, the analysis of profit, this may not matter much. It is just as possible to make profit by the correct anticipation of exogenous change (by speculation) as from change deliberately brought about (by enterprise). But this distinction, while possibly irrelevant to the theory of profit, is highly relevant to the theory of entrepreneurship. If, therefore, Professor Knight accords entrepreneurial status to the shareholders on the grounds that "the final control is the selection of men to control in business organisation, and this is inseparable from responsibility" while refusing it to the salaried manager because "whenever we find an apparent separation between control and uncertainty-bearing, examination will show that we are confusing essentially routine activities with real control,"[5] he is, in our opinion, himself guilty of confusing the selection of persons to be entrusted with the task of carrying out changes with this activity itself. The mode of the latter is in no way determined by that of the former.

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[1] Italics in original.

[2] On the sociological basis and background of Company Law see Mises: *Socialism*, pp. 208–10.

[3] F. H. Knight: *Risk, Uncertainty, and Profit*, 1933 re-issue, p. 35.

[4] This distinction is mentioned on p. 316, but the point is not developed any further in its bearing on entrepreneurship.

[5] Op. cit., p. 298.

Enough, we trust, has been said to convince the reader that the location of entrepreneurship in modern joint-stock enterprise is as yet an unsolved problem. It is as a contribution to this problem that we have to look at the theory of Finance Capitalism. This theory avers that at a relatively late stage of the evolution of capitalism the active, planning and decision-making function in industry tends to devolve upon the representatives of financial interests. Prima facie it is not unplausible. Since the abdication of the modern shareholder there is a vacancy on the throne of joint-stock enterprise. It is not in conflict with economic theory, for economic theory has nothing to say about it. Whether it is not only plausible but true is the question to which we now have to address ourselves.

VERIFICATION

Any adequate empirical verification of the thesis under discussion would require nothing short of a comprehensive economic survey of the modern World. All we can do here is to examine a few facts from the recent economic history of the three industrially most advanced countries, Britain, Germany and the United States—the countries, that is to say, to which alone our thesis can apply—with regard to their relevance to our problem. The thesis under discussion, let us remember, is that in modern history the entrepreneurial, planning and decision-making function tends to shift into the hands of " financiers ", of the intermediaries of the capital market who specialise in directing capital flows. (This thesis will henceforth be referred to as " the thesis " or " the thesis under discussion.")

Wherever we may have to look for facts to bear out the thesis, British industry is not the place to look for them. On this point the strictures of the Macmillan Report[1]—especially in Part II, chapter IV—on the lack of contact between British finance and industry[2] are not yet forgotten. Nor can it be said that in this respect much has changed since 1931. It is certainly true that in the 1930's British industry has found the capital market far more accessible than before, but that has entailed none of the features of Finance Capitalism. A country to which an institution like the Bankers' Industrial Development Company came as a novelty, the terms of its intervention over the Ebbw Vale project in 1938 as something like a shock, and the resignation of Sir William Firth from the chairmanship of Richard Thomas & Co. and the events culminating in it as almost a financial sensation, is certainly no place to look for the manifestations of Finance Capitalism. All this, however, applies only to British home industry. Where British overseas enterprise is concerned the story is very different. As regards overseas mining enterprise, for instance, " London has for many years been a great centre for the organisation and finance of mining enterprise. . . . There has been slowly built up in England over the past 100 years or so a very efficient, very complex organisation, financial, technical, industrial and commercial, which is spoken of as the London Mining Market. It is engaged in mining exploration in overseas countries, and in financing, developing and managing mining enterprises."[3]

As regards British industrial enterprise as a whole (both internal and overseas), the evidence for the existence of Finance Capitalism must therefore be regarded as inconclusive.

Prima facie the German case looks more favourable to our thesis. It is, of course, well known how much of German industry has been " nursed " by banks. Moreover, Dr. Hilferding and Lenin wrote with German examples in their minds. The prominent part played by the bankers of Cologne (like the A. Schaaffhausen'sche Bankverein) in establishing the industries of the Ruhr district is a notable example. But we have to remember that according to our thesis Finance Capitalism belongs to a late stage of capitalist evolution. Financial control over industry at the very beginnings of German industrialisation is therefore no evidence for it. What evidence there is seems to point in the opposite direction.

In his careful and penetrating study of German banking (before 1930) Mr. Whale observes that " on the whole there was a tendency before the (last) War for the larger industrialists to become less dependent on the banks and to exercise greater initiative in their own sphere".[1] This certainly applies to the "heavy" industries of the Ruhr which about the turn of the century had reached economic maturity.[2] It is even more interesting to observe that the chemical and electrical engineering industries, which after 1900 were Germany's two outstanding "young" industries, never fell under financial tutelage, even during the period of their economic immaturity.[3] In the chemical case this was most probably due to the high monopoly profits which allowed the industry to expand almost entirely out of its undistributed profits. But in the instance of electrical engineering it was due to the financial genius of Emil Rathenau[4], who devised a special kind of holding company (*Finanzierungsgesellschaft*) for the financing of the public utilities on the development of which the progress of his own company, the A. E. G., largely depended, thus retaining the central direction of the process of electrification while yielding to the bankers the control of the results of the process.[5] All this seems to bear out Mr. Whale's conclusion: "With regard to the question of leadership, it is always difficult to know how much is to be ascribed to personality and how much to circumstances. But it does seem significant that whereas in the period from 1850 to 1880, or thereabouts, the most outstanding men in business life were bankers—Hansemann, Mevissen—in the following years they were industrialists—Rathenau, Kirdorf. In part at least this may be explained by the fact that the banks, having played their pioneer rôle, were now becoming more stereotyped in their business, and thus offered fewer opportunities to men of initiative."[6] This conclusion, of course, openly contradicts the thesis. It must be added, however, that in the 1920's, after inflation and stabilisation, there appeared certain phenomena which lend themselves to an interpretation in terms of it.

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[1] *Report of the Committee on Finance and Industry* (Cmd. 3897), 1931.

[2] The historical reasons for this lack of contact are stated with admirable precision in paras. 375–77. For the strictures see mainly paras. 385–90.

[3] Lord Geddes in his address to the General Meeting of the Rio Tinto Co., May 21, 1943.

The United States is widely regarded as the Mecca of Finance Capitalism. On this point economists as different in background, sympathies and outlook as Thorstein Veblen[7] and the authors of the Macmillan Report are agreed.[8] More particularly, in the American mind our phenomenon is usually associated with that sphere of industrial influence which during the decades of economic expansion some prominent New York banking houses have been able to acquire and extend. It is hardly an exaggeration to say that to four out of five Americans Finance Capitalism is synonymous with the ascendancy of J. P. Morgan & Co. over a large field of American industry.

An exhaustive study of the relations between J. P. Morgan & Co. and the American Telephone & Telegraph Company (A.T. & T.), America's largest industrial corporation (total assets over 4 milliard dollars), has been undertaken by the Temporary National Economic Committee (T.N.E.C.). In an investigation extending over several weeks, and in cross-examinations conducted by one of the ablest counsels of the Securities and Exchange Commission, nothing more germane to our problem came to light than attempts to monopolise the sale of A.T. & T. bond issues. Even here Mr. Whitney, a partner in J. P. Morgan & Co., was able to point out that whatever influence his firm may have had over A.T. & T. financing was confined to bond issues, and to refer to "the well-known fact that during that period [1906–39] they [A.T. & T.] sold vast amounts of common stock, generally to their own stockholders, and a certain amount of convertible bonds during that period, all to their own stockholders, without any underwriting, and I think it is a fact that they increased the capital stock during this period something like 10 times without any relation to the bankers. . . . I think you will find if you check the records . . . that substantially more than half the total additional financing done from 1906 down to the present day was done through stock offered to their own stockholders, always at par and without underwriting of any kind."[1] [2] [3]

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[1] P. Barrett Whale: *Joint Stock Banking in Germany*, Macmillan, 1930, p. 64.

[2] The situation as it had then developed is well illustrated by a remark which August Thyssen is said to have made during the crisis of 1900: "I sleep much better than my bankers in Berlin. They have lent me 50 million marks. I only owe it to them." Se non è vero, è ben trovato.

[3] On the development of the German chemical and electrical engineering industries see Hermann Levy: *Industrial Germany*, Cambridge University Press, 1935, Chapters IV, V and VI.

[4] For a brilliant pen portrait of him see Gaston Raphäel: *Walther Rathenau*, Payot, 1919, pp. 15–28.

[5] For particulars of his method see Whale, op. cit., pp. 57–9.

[6] Whale, op. cit., p. 64.

[7] "American business is eminently of a financial character, and the traffic of these financiers runs within the closed circuit of money-market strategy, with any industrial effects of this financial management coming in as incidentals. The controlling incentives are those of the market for securities, not those of the output of goods; and the final discretion rests in the investment banker, not in the engineering staff or the manager of the works. The discretionary direction of affairs has in effect passed into the hands of these financiering houses, whose ostensible relation to the industrial concerns is that of underwriters only." Thorstein Veblen: *Imperial Germany and the Industrial Revolution*, New Ed., New York, 1939, Supplem. Note IV, pp. 339–40.

[8] Macmillan Report, para. 380.

Practically all the bankers who gave evidence before the T.N.E.C. described their relationship to industry as the rendering of services in financial matters. Most of them emphasised the quasi-professional character of these services and added that the initiative lay invariably with their industrial clients. This point has found its classical formulation in the evidence which the late Otto H. Kahn gave in 1933 before the Pecora Committee: "(A doctor) gets his clients by reason of his reputation for ability and for successful cures and for sound advice given. And so it is with the lawyer. So it is with the architect. And so in our case it has long been our policy and our effort to get our clients, not by chasing after them, not by praising our own wares, but by an attempt to establish a reputation which would make clients feel that if they have a problem of a financial nature, Dr. Kuhn, Loeb & Co. is a pretty good doctor to go to."[4] Testifying before the T.N.E.C., Mr. Whitney of J. P. Morgan & Co.[5] and Mr. Mitchell, President of Bonbright & Co.[6] used similar analogies between legal and financial advice. We must, of course, not forget that these are *ex parte* statements of the case.

But the T.N.E.C. investigation also revealed that the relationship between investment banker and industry is not always of the simple, quasi-professional type,[7] and that in fact there are cases in which bankers try to influence industrial management and managerial appointments. The case of the Cleveland-Cliffs Iron Company, on which the issuing bankers imposed fairly stringent conditions as to choice of officers, when in 1935 it converted a large bank debt into a bond issue, gave rise to the following exchange of views between the the chairman, Senator King, Mr. Leon Henderson (then) of the Securities and Exchange Commission, and the witness, Mr. B. A. Tompkins, Vice-President of the Bankers' Trust Co.:

> Chairman: Would not a reputable investment house that sells its bonds to the public, even though it doesn't put up the money itself but sells bonds and gets the money for its patron, be interested in having the company whose bonds it was selling or underwriting operated in a judicial manner and by persons competent to deal with the problems that would arise in the administration?
>
> Mr. Tompkins: Absolutely.
>
> Chairman: Therefore, is there anything improper, in your judgment, and has it not been the practice not only with private individuals who are loaning money, but investment companies and banks, to inquire into the character of the business and who was in charge of it, and to desire to be satisfied as to the competency of the persons in charge to discharge their duties and obligations so as to make the business a success?
>
> Mr. Tompkins: That is always a very important consideration in handling money.
>
> Chairman: So the investment company, I would suppose, would feel its honour was more or less involved when it sold securities to the public, though it did not advance its own money?
>
> Mr. Tompkins: Yes; it has a continuing responsibility to the people who buy the securities.
>
> Mr. Henderson: You get into a number of very delicate questions, do you not, when you get into the position of passing on management and its

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[1] *T.N.E.C. Hearings*, Part 23, p. 11847.

[2] At no time later than 1905 had the 20 largest shareholders more than 11 per cent. of outstanding stock. Whatever control may have been exercised over the A.T. & T., it certainly was not located in the majority of stock.

[3] Professor R. A. Gordon, who has examined 155 of the 200 largest industrial corporations in the United States, thinks "It is significant that there were practically no very large (10 per cent. or more) holdings by banks (none by investment banks). This is not to say, of course, that banker control may not exist. Such control, in so far as it does exist, merely does not seem to need the reinforcing power of ownership." (R. A. Gordon: "Ownership by management and control groups in the large corporation," *Q.J.E.*, May, 1938, p. 385, n. 4.)

[4] *Hearings before the Senate Committee on Banking and Currency*, Vol. 3, pp. 959–60, June 27, 1933.

[5] T.N.E.C., *Hearings*, Part 23, p. 11858.

[6] Loc. cit., pp. 12094–5.

[7] For instance, it was pointed out by Mr. Whitney that the relationship between the Morgan bank and some of the railroads was very different from that with the A.T. & T.

judgments. You get into what they call banker management once in a while, do you not?

Mr. Tompkins: Yes. Management itself is a thing that a banker tries to avoid. He feels that his responsibility is to help select management, but wherever, in our experience, I have seen a banker as a banker try to operate an industrial company, the record of his management generally indicates that he ought to have stayed in the banking business.

Mr. Henderson: That isn't always true, though. There are some examples where the bankers took over successfully.

Mr. Tompkins: There are some examples.

Mr. Henderson: But I think I would incline to agree with you that they ought to stick to their banking.

Mr. Tompkins: In general.[1]

We may therefore conclude that the relationship between financial and industrial enterprise is of such a complex nature as to defy easy generalisations.

INTERPRETATION

Our attempt at an empirical verification of the thesis under discussion has not been successful. We endeavoured to " let the facts speak for themselves "—and the facts said ever so many things, and said them in a somewhat ambiguous fashion. Some of the phenomena which we have observed bear an interpretation in terms of our thesis, others do not. Clearly, then, as our next step, we have to establish a criterion of distinction between the two classes of phenomena. Such a criterion has to satisfy two conditions. It must be sufficiently general to be of theoretical interest; unless it is on the same level of abstraction as economic theory in general we cannot fit it into our scientific edifice. But, secondly, it must not be purely formal: unless it helps us, not only to distinguish classes of phenomena, but *to understand why* the phenomena are different, it will make no contribution to the progress of our knowledge.

The thesis under discussion is a proposition about a social process. A social process consists of a number of mutually interrelated human actions. To analyse a social process means to explore the mode of this interrelationship, to dissect the process into actions. To understand an action means not only to know what it is " for ", its purpose, but also to know the plan behind the action, to reduce " action " to " plan ". In analysing a social process we have to start with the plans of the individuals, for it is their simultaneous attempts to carry out these plans—which may be inconsistent with each other and are to that extent bound to fail—which give rise to the process and determine its pattern. In analysing the process with which the thesis is concerned, we are studying the relationship between financial and industrial enterprise, and in tracing the pattern of financial action we shall learn what problems it sets out to solve.

The investment banker is a merchant of securities.[2] Being a merchant he tries to maximise his turnover. It is not, however, the total turnover of existing securities, but the number and magnitude of new issues forthcoming under his auspices that is of interest to him. And since new issues of securities will roughly (and with a few exceptions, one of which is important and will be discussed) represent new investment, it will usually be possible to say that the main source of the demand for his goods is saving, the main source of his supply new investment. A unit of production interests him solely in so far as it is, or is likely to become, a source of supply of those securities which he sells. Being, like every trader, by instinct a monopolist, he will try to secure for himself as large a number of supply sources as possible. But the efficacy of units of production as supply sources of securities depends on their capacity for expansion. Those that offer most extensive " investment opportunities " will be most attractive to investment bankers.

Investment opportunities are never simply " there "; they are the result of human action, the outcome of a process in which will-power and intensity of effort play a most prominent part. The number of them that can at each moment be regarded as " data ", waiting for a falling rate of interest to bring them into the limelight of industrial actuality, like pebbles on a beach which are exposed to the effects of light and air by the receding tide, is generally insignificant. Most investment opportunities owe their existence to innovations; they are creations of the human will, manifestations of enterprise. A " lack of investment opportunities " will therefore indicate nothing so much as lack of enterprise. The innovations in which enterprise manifests itself may either take the form of changes in the combination of factors within a given unit of production (firm), or of the creation of new units, or of a modification of the relationship in which these units stand to each other, the " structure of industry ". For fairly obvious reasons industrial enterprise will find least difficulty in accomplishing the first, more in achieving the second, by far the most in the third kind of innovation.

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[1] *T.N.E.C. Hearings*, Part 24, pp. 12465–6.

[2] The reader is asked to bear in mind that in what follows we are exclusively concerned with investment banking, not with deposit banking.

Precisely at this point, where industrial enterprise falters, financial enterprise enters the stage. In a progressive economy the financier has a function to fulfil. In order to describe this function we have to show, firstly, why it is a necessary function, secondly, why the financier is capable of fulfilling it, and thirdly, why it is in his interest to do so.

As to the first, it is obvious enough that a progressive economy is characterised by constant innovations which will frequently necessitate modifications of the industrial structure, the setting up of new industries, the reorganisation of declining trades, etc. Confronted with such problems, industrial enterprise often fails, sometimes because they are beyond the reach of the management of a single firm, but sometimes for sheer lack of initiative, absence of strong business leadership, in short, for want of those human qualities which are indispensable to successful enterprise.[1] Secondly, in practically every instance innovation requires capital investment. (This is, of course, merely another way of saying that in a progressive economy innovation provides the main investment outlet for savings.) Now, the investment banker is a specialist in directing capital flows. By deflecting and withholding such flows he decides which investment opportunities will become actual innovations. Thirdly, since he is a merchant of securities, it is to his interest that savings do not run to waste, but find an outlet in investment and its financial counterpart, the flow of new securities. It is therefore to his interest to keep the flow of new securities on a level with the flow of savings, and to see to it that this level remains as high as possible.

To the rule that the source of new securities is new investment, there is an important exception, viz. the reorganisation of businesses which have suffered capital loss. To this day economists have paid surprisingly little attention to the problems presented by bankruptcy and company reconstruction. The reason for their lack of curiosity has probably to be sought in their excessive preoccupation with the more spectacular (and measurable !) " macrodynamic " aspects of capital. As long as changes in the amount of investment remain the only kind of capital change that can attract our attention, we are unlikely to discover the nature of the processes going on *within* that heterogeneous assortment of production goods with multiple uses. We shall have nothing to say, for instance, about *internal capital change*, or about the causes and consequences of malinvestment. It is precisely here that financial enterprise impinges upon our problem.

The reorganisation of companies which have suffered capital losses is a kind of economico-surgical operation which requires the skilled hand of a specialist. The establishment of a business denotes the combination of various producers' goods (plant, equipment, raw materials, etc.) with a sum of money in such a way that the fixed resources, as it were (in the short run which here alone concerns us), form a receptacle for the circulating capital. Where this combination proves incapable of producing a net return over operating costs, the flow of circulating capital will be deflected from our combination of fixed resources, the receptacle will become empty, an example of " excess capacity " presents itself. The solution of the dilemma evidently lies in transferring the fixed resources to some other use, " to another industry ", either bodily and " as a going concern " (as, for instance, when an hotel with all its furniture and equipment is turned into a nursing home), or, perhaps, by partly dissolving the combination and changing its composition (as in the case of an unsuccessful theatre being converted into a cinema, where the stage settings become superfluous and can be disposed of by sale, but the combination has to be supplemented by the installation of a projector and screen). As error is a concomitant of rapid change, the recurrent necessity for such economic surgery is a regular and prominent feature of economic progress.

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[1] A country in the first stages of industrialisation will often suffer from a lack of industrial enterprise and business experience which may well be its scarcest factor of production. In this case the task of starting new industries, one after the other, may devolve upon the bankers (and sometimes other merchants) as the specialists in engineering economic change. For a free enterprise economy adjusts itself to new problems by evolving a kind of professional problem-solver (" . . . that if they have a problem of a financial nature, Dr. Kuhn, Loeb & Co. is a pretty good doctor to go to . . ."). It pays him handsomely and relieves him from all non-specialist work. And the ignorant will firmly believe in the existence of the idle rich.

Such an economico-surgical operation requires the re-directing of the capital flow; somebody has to fill the new receptacle. The nature of the problem thus clearly points to a specialist in the directing of capital flows as the surgeon most likely to succeed.[1] Moreover, it is a task requiring a concentrated general knowledge of the whole economic system (such as one frequently finds in export merchants) surpassing that of the ordinary industrialist whose knowledge is necessarily specialised and restricted to *his* market, his plant, and to his industry. It is whenever problems of this kind have to be solved that the entrepreneurial function shifts into the hands of financiers. The *haute finance* is, as it were, the second line of defence of a free enterprise economy faced with problems of intersectional maladjustment. Where the task proves too intricate for ordinary industrial management the financier "takes over".[2] In the vernacular of Hollywood, he is the "trouble shooter" of Capitalism. This fact, once it is grasped, permits us to infer that a "vanishing of investment opportunities" indicates nothing so much as a cracking of the second line of defence.

The criterion of distinction between cases in which the entrepreneurial function does, and those in which it does not, shift is therefore found in the appearance or non-appearance of problems with which industrial entrepreneurs are unable to cope. Finance Capitalism, we may conclude, is not an epoch of economic history. It denotes the way in which a free enterprise economy adjusts itself to certain necessities when they arise, a type of response to problems, and a mode of solving them. There is no reason why we should not regard it as a recurrent phenomenon.

CONCLUSION

The thesis under discussion in this paper predicates the occurrence of a phenomenon as distinct in time from other phenomena. It suggests a view of economic history as a continuous succession of "phases", each of which is more or less dominated and characterised by such a phenomenon and each of which is, by virtue of this fact, distinct from all other "phases" preceding or succeeding it. In our particular case, the thesis asserts that the last phase of economic evolution in that part of the World that is industrially advanced and about which it is possible to generalise from experience—that is, roughly speaking, the half-century preceding the outbreak of the Second World War—was, in the sense mentioned, an age dominated and characterised by "Finance Capitalism".

What fragmentary evidence we were able to gather failed to support the thesis, at least in its historical aspect. A number of instances of "Finance Capitalism" were found, but they did not conform to the time pattern postulated. In British industry, as late as for the 1930's, no such instances could be discovered, while in British overseas enterprise they could be traced back a very long time. In Germany instances were found at the very beginnings of her industrialisation and seemed to disappear later on. But we also noticed that in the 1920's, after inflation and stabilisation, they showed a tendency to reappear.

We then applied to the phenomena observed the ordinary method of economic analysis which regards all events as the outcome of the pursuit of—possibly rival—human plans. It is true that in traditional static theory this method has hitherto only found a very limited range of application, confined to those cases in which the purpose pursued by individuals is to "maximise" something. But it is readily seen that this is an entirely arbitrary restriction of the method, and that its potential range is co-extensive with all human activity which is conscious of its purposes, in other words, which is problem-solving activity and therefore capable of rational explanation; strategy is an obvious example where the method is in fact applied.

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[1] It was when confronted with such tasks that there excelled the financial genius of Sir Ernest Cassel. A comprehensive study of his career from the point of view here indicated would perhaps throw more light on the problems of "capital re-grouping" than many learned volumes on the applied theory of capital.

[2] But, of course, it may be that the task is beyond his capacity. "In Britain after the (last) war the intervention of bank capital was held off as long as possible, but in the 'twenties, when iron and steel prices were below the general price level and falling continuously, debentures fell more and more into the hands of the banks and they were forced to take an interest in the industry. A large part of the firms were living on frozen overdrafts and under the legal control of the banks, which did not possess the experience to exercise positive direction." T. H. Burnham and G. O. Hoskins: *Iron and Steel in Britain 1870–1930*, London, 1943, p. 263.

In studying the complex of phenomena described as " Finance Capitalism " we were able to approach our subject from a new angle and to discover traits which thus far had gone unnoticed. We asked in the pursuit of what purpose it comes about that industrial entrepreneurship shifts into the hands of the specialists in directing capital flows, what problems present themselves to the minds of those concerned, what are the conditions of their successful solution. Finance Capitalism, when seen from this angle, emerges as a type of action designed to meet a certain situation, an attempt to solve problems which for all we know about them need *not* be phenomena distinct in time but may well be recurrent phenomena.

We must leave it to the reader to judge whether in this way a deeper insight, a more satisfactory understanding of the problem has been achieved. But it is evident that his judgment will have an important bearing not only on the relative merits of " methodological individualism " and " methodological collectivism ", but also on the issue which formed our starting-point in this paper, viz. the mode of co-operation between economic theorists and historians. As Professor Hayek has repeatedly pointed out,[1] there is no inherent antagonism between those who evolve abstract patterns of human action and those who use them for the explanation of concrete events; the two methods are in fact complementary. There can, of course, be no historical narrative which is not at least an attempt at a rational explanation of the events narrated, for it is impossible to give any intelligible account of social phenomena in any other way. The greatest obstacle to closer co-operation between theorists and historians seems to lie in the fact that theorists find it difficult to produce models with time-dimensions, while historians cannot apply models which have no time-dimensions to the explanation of real processes which have. Moreover, one does not have to be a Bergsonian to feel some doubt whether the purely chronological concept of Time which historians seem bound to employ is necessarily the last word in the refinement of method which a social science is able to attain.

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[1] " Scientism and the Study of Society," II, ECONOMICA, February, 1943, pp. 57–8. " The Facts of the Social Sciences," *Ethics*, October, 1943, p. 9, n. 4.

The Duties of a Banker—II

THE EFFECTS OF INCONVERTIBILITY

By J. K. HORSEFIELD

> " . . . events of a calamitous trend (which in the imagination of some took the form of a swarm of migratory dragons and with others was represented by a depreciation of the currency) . . ."
>
> *Kai Lung Beneath the Mulberry Tree.*

I

AN article published three years ago[1] examined some of the views current at the end of the eighteenth century on the responsibility of bankers for the total of their notes, as a first step towards analysing the controversies behind the Bank Charter Act, 1844. It was then suggested that the error, or at best ambiguity, of the crucial passages in the *Wealth of Nations* conduced about 1790 to an over-facile belief that—crudely stated—any continuing excess of note-issues above a safe figure was automatically prevented—a comfortable theory reminiscent of John Law. Such a doctrine, which we called the " Smithian " theory to distinguish it from the more sophisticated version later called the " Banking Theory ", was dangerously encouraging to a banking community which was far from experienced.

Smith's views did not, however, apply without qualification to the Bank of England, because they were based upon the proviso that notes were issued only against sound commercial discounts, whereas the Bank was constantly a creditor of the Government. As a discounter of Exchequer paper, the Bank could find no guidance in the pages of the *Wealth of Nations*, and its actions to restrict the note-issue in 1783, 1793 and 1796 seem to have been dictated by instinct, or at best by empirical experience. Moreover, such independent vision as it possessed may have been due to the passing influence of one or two unusually clear-sighted Governors; there was, at least, a significant difference between the criteria of danger accepted in 1783 (the level of the foreign exchanges) and in 1797 (the state of the Cash). The latter change explains why, once the Restriction Act had rendered the Cash inviolable, the Bank appears readily to have succumbed to the view that all was necessarily well with the note-issue; and when called upon in 1810 to expound its views could only produce a " Smithian " defence, which was irrelevant because Smith was discussing only *convertible* notes.

It is the purpose of the present note to carry the story a stage further, from the Suspension of Cash Payments on 26th February, 1797, until the end of 1804. To follow the developing theories through this period necessarily involves reconsidering literature which has already been worked over so often and so comprehensively[2] that it is difficult to avoid sheer repetition. It is hoped, however, that as our interest lies less in the theories actually expressed than in their relationship to those which preceded and succeeded them, we may be able to avoid mere plagiarism. More can hardly be expected to-day of a writer seeking once more to traverse the happy hunting-ground of every monetary specialist since Jevons.

II

The new element introduced by the Suspension into the discussion of monetary topics was, of course, that the currency had become inconvertible. We may distinguish three sub-periods during which this factor came gradually to dominate and to clarify current theorising. Thus in 1797, under the immediate impact of the Suspension, the subject chiefly debated was whether bank notes, being precluded from encashment, should be made legal tender. In 1800–1 attention was diverted to the possible responsibility of inconvertible paper for a sudden rise of prices, which also brought in the question of country banks' note-issues and their relationship to those of the London banks and the Bank of England. From this emerged, incidentally, the early symptoms of that almost ultramontane attitude towards the Bank which distinguished many of its defenders during the later controversies. Finally, between 1802 and 1804 there clearly grew up that clash between contradictory views about the control of currency and credit which was to dominate monetary discussion for half a century. We shall deal briefly with the first two of these periods; then attack the more complex issues raised in the third; and finally consider separately the position of the Country Banks.

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[1] ECONOMICA, February, 1941, p. 37.

[2] E.g. Silberling, *Quarterly Journal of Economics*, 1924, pp. 214–33, 397–439. Viner: *Studies in the Theory of International Trade*, pp. 119–170. Hayek: Preface to his edition of Thornton's *Paper Credit*, pp. 36–58.

The question whether Bank of England notes, while inconvertible, should be made legal tender, was for a short time a burning issue. In a letter[1] written at 2 a.m. on February 27th, 1797, to an intimate of Pitt's, Walter Boyd had besought the Government not to take this step; the experience of France since 1789 showed only too clearly that convertible notes alone were safe, and to concede legal tender qualities would fatally underline the dangers of the Suspension.

Even stronger fears were expressed elsewhere—for example in a pamphlet published in Bristol shortly afterwards.[2] The writer envisages the complete collapse of the Bank's credit. Bank notes would never regain their extensive circulation in Europe and the Empire. Associations to support the Bank would be useless, since paper money would soon cease to maintain its nominal value, and before long every article would sell for two prices, one for specie and one for paper. To make Bank notes legal tender would, he considered, only be a " more certain means of causing their downfall ". The only remedy was to abandon the French War and all projects of conquest, and restore, before too late, the convertibility of the note-issue.

Against this jeremiad we may set the more reasonable optimism of Sir Francis Baring. His *Observations on the Establishment of the Bank of England*, written later in 1797, was intended partly to explain the Restriction, but partly also to advocate that legal tender qualities should be conceded. He contended that although *Government* paper (witness the assignats) was unsuitable for legal tender, Bank Notes could be given this quality not only with safety but to advantage.[3] He wished to stabilise the position of the Bank so as to avoid any possibility of a premature Resumption during the War, which might lead to a second Suspension.

This conception of a prolonged Restriction was, however, not at all widely shared. In actual fact, the change to inconvertible paper was made so smoothly as to confound the pessimists—and incidentally, perhaps, to lead to underestimates of the importance and dangers of the change. The credit of the Bank's notes was completely maintained, and by March 17th—less than three weeks after the Suspension—Francis Horner, then in London, was able to write to his father that " the panic is now in great measure worn off, I hope from no other cause than it being discovered to have been unfounded ".[4] The subsequent " numerous meetings of merchants ", which assembled weekly at the London Tavern under the chairmanship of Alderman Lushington, confined themselves to urging an extension of commercial discounts.[5] By April 21st, when the House of Commons Committee on the Outstanding Demands at the Bank presented its Third Report, the new situation was wholly accepted.

Looking back from the safety of July, 1798, " A Proprietor of Bank Stock " thus summarised the transition:—" In this desponding state, when all men dreaded, with the utmost anxiety, the event that was seen to be inevitable, and not far distant, and which it was supposed would involve the Kingdom in general bankruptcy and intire ruin, the 26th February, 1797, was the crisis that gave the happy turn, and almost instantly dismissed all the horrors and fears that surrounded us; restored complete confidence . . ."[6]

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[1] Reprinted as Appendix C to Boyd's *Letter to Rt. Hon. Wm. Pitt.*

[2] *Cursory Reflections on the Causes and some of the Consequences of the Stoppage of the Bank of England*, by Edward Long Fox, M.D. The preface is dated 16th March, 1797.

[3] P. 68.

[4] *Life.* I, p. 35.

[5] See Minutes appended to Evidence taken by House of Commons Committee on Outstanding Demands (3rd Report, 1797, Appendix 18).

[6] *A Letter to the Proprietors of the Bank of England on the state of their profits and accumulation* ..., p. 8.

As a result, no doubt, the Restriction Act[1], passed on May 3rd, declared Bank Notes to be legal tender only at the Bank and for payment of taxes. The relevant passages of the Act were section 2, restraining the Bank from giving cash for its notes; and sections 8, 9 and 10, which provided respectively that payment in Bank notes was to be deemed payment in cash if accepted as such; that a tender of bank notes was to be sufficient to avert imprisonment for debt; and that collectors of the public revenue were to accept them as cash.

These provisions appear to have been accepted with little difficulty. It is true that Baring complained[2] that "Bank Notes . . . were not [made] a legal tender between individuals, who were given to understand that if they were compelled to have recourse to a court of law, by means of sophistry and quibbling, they should have — — not relief — — but something which was never distinctly explained, as it never was correctly understood." But there is no sign that the Courts were overburdened with attempts to evade the Act, such as might have been expected if it was generally unpopular. Litigation appears to have been frequent only over the provisions of section 9; various cases in 1800 and 1801 tested the Courts' interpretation of the requirement that affidavits supporting the arrest of debtors must state categorically that no offer of payment had been made in bank notes. [It was characteristic of the time that affidavits by a clerk denying an offer in respect of a debt due to his employer were accepted as sufficient by the King's Bench,[3] but refused as inadequate by the parallel Court of Common Pleas.][4]

A recorded case[5] in November, 1801, where an attempt was made to challenge the whole principle of the Act, is, for once, an exception which really proves the rule. From observations made by the Judges it is clear that the English genius for compromise had already achieved, without help from the Courts, an illogical but entirely satisfactory *modus vivendi*. The facts of the case were that one Grigby requested a banker at Bury St. Edmunds, named Oakes, to cash one of his (Oakes') notes for £5 5s. 0d. Being offered five shillings in silver and a Bank of England note for £5, he refused the latter and demanded to be paid "wholly in money". It was evidently a test case, for the report continues: "He stated that he came on purpose to have cash for the note, or to bring an action if payment in money was refused." Counsel for the defendant argued valiantly, but vainly, that if the plaintiff refused to accept Bank notes the defendants might be taken in execution through being unable to find enough cash, which was inconsistent with Section 9. Judgment went to the plaintiff, who had on his side the plain words of the statute, but the Lord Chief Justice did not allow him to depart unscathed. After remarking that the Act did not intend to prevent any creditor who should be so disposed from "captiously demanding a payment in money", he went on: "Thank God few such creditors as the present plaintiff have been found since the passing of the Act!"[6]

Looked at in the context of the larger monetary issues, we may see in these arguments about legal tender a dim groping away from the concept that only the Crown could create currency, towards the idea that the monetary system was something to be managed as a whole. In later years echoes of the controversy reappear, as for example in suggestions that the Government ought to issue its own notes instead of relying on the credit of the Bank of England.[7] But the *fait accompli* was less and less questioned as public interest was diverted to other topics.

III

In 1800 public attention became focused on a sudden increase in the cost of food. Two successive failures in the wheat harvests sent the *Gazette* price of wheat, which was 49/6d. in January, 1799, up to 134/5d. in June, 1800; it was still at 129/8d. in June, 1801.[8] Contributory factors were the obstructions placed by Continental powers on the export of wheat to this country.[9] As a result, Silberling's weighted cost-of-living index (1790 = 100), although showing a total rise from 1797 to 1804 of only 20 points, from 120 to 140, moved between 1798 and 1802 as follows:—

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[1] 37 G. III, c. 45.

[2] *Op cit.*, pp. 66, 67.

[3] Mayor of London v. Dias (1 East 239).

[4] Smith v. Tyson (Bos. and Pull. II, 339); Hammersley v. Mitchell (Bos. and Pull. II, 389).

[5] Grigby v. Oakes and Anr. (Bos. and Pull. II, 526)

[6] Lord Alvanley, at p. 528.

[7] E.g. Howison: *An Investigation into the Principles and Credit of the Circulation of Paper Money* (1803), p. 29. Magens: *An Inquiry into the Real Difference between Actual Money . . . and Paper Money*, 1804, p. 66.

[8] Tooke, I, pp. 212, 216, 237.

[9] *Ibid.*, pp. 224ff.

1798 = 121
1799 = 143
1800 = 170
1801 = 174
1802 = 138[1]

These fluctuations are much greater than the concurrent movements in the unweighted wholesale index, which varied only between 143 and 166,[2] and agricultural wages were even stickier, moving between 148 and 162 only.

As a result of this upheaval, the years 1800 and 1801 produced a crop of pamphlets, mostly bad, which sought to place the blame not only on the defective harvests, but also on forestallers, regraters, the Government or the currency.[3] The problem was discussed in the House of Commons in November, 1800, but only passing reference was then made to the possible responsibility of the Bank—a suggestion which Pitt rejected on the grounds that the increase in the volume of currency since 1797 was inconsiderable. In the same month,[4] however, Walter Boyd attempted, in his celebrated *Letter to the Rt. Hon. William Pitt*, to demonstrate that the Bank was wholly to blame. He attacked it, as a matter of fact, no less for causing grave commercial embarrassment by over-stringency in 1795–7 than for causing inflation by over-issue since; but we are concerned now only with the second accusation. The first edition of this book was written in ignorance of the Bank's actual note-issue, and merely inferred a considerable increase in the notes in circulation from the existence of the rise in prices and in the cost of bullion, and from the fall in the foreign exchanges. "Finding these strong symptoms in common with so many others," he writes, "I am fully warranted to ascribe them to the same great and general cause."[5] But shortly afterwards the Bank's circulation for December 6th, 1800, was made public, and in a second edition Boyd rashly seized on the fact that this showed a considerable increase above the figure for February, 1797, overlooking that the totals were not really comparable. In the first place, the 1797 figure was abnormally low; in the second, Boyd's comparison made no allowance for the fact that by 1800 some £2 million in small notes was in circulation, not as an addition to, but as a substitute for, the vanished gold.

The defects in this quantitative argument were promptly exposed by Sir Francis Baring, who was anxious, in 1801 no less than in 1797, to defend the Bank. Baring himself, however, pursued his case to an inadmissible excess, for from arguing that internal prices do not necessarily vary with the course of exchange (as Boyd had suggested they must do) he went on to say: "The course of exchange has, therefore, no connection with the price of provisions grown and consumed in this country; nor can I guess in what manner Bank Notes, which circulate at par [he means, on an equality with gold] can contribute, by any manner of means to raise the price."[6]

The confusion of thought in this paragraph is obvious; its irrelevance becomes apparent when it is realised that by 1801 gold bullion had actually begun to show a premium, so that disparate prices were, in fact, already in existence. On the other hand, Baring's general attitude towards the Bank remained on this side idolatry. A clearer foreshadowing of the developing Banking Theory is found in minor pamphlets—for example one by T. S. Surr (1801) which contains the dogmatic assertion that it is impossible that the Bank of England should be responsible for issuing notes which have lost their value, because they are issued only in discounts (after scrutiny of the security), in purchase of bullion, paying dividends, and against Government securities.[7]

This discussion of the responsibility for increases in prices acted, in fact, as a catalyst, precipitating from the self-contradictory dogmas of Adam Smith the two separate trains of thought which became the Currency and Banking Theories. That the time was ripe for this clarification is obvious from a gradual change in the attitude towards Smith's work, a reverent acceptance of which had been common form at the end of the eighteenth century.[1] So far, the more critical attitude which succeeded was limited to a select few, but in Scotland it was finding cautious expression in the lectures of Dugald Stewart. In London, as one of Stewart's pupils, Francis Horner, found on migrating there in 1803, no corresponding centre of interest in political economy existed.[2] It is true that if he had penetrated to Clapham, he would have found there a mind at least equal in independence and clarified judgment to his preceptor's; but it was left to Horner himself, having publicised " Paper Credit " in the Edinburgh Review, to force upon a reluctant House of Commons, in after years, the consideration of Thornton's views.

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[1] *Review of Econ. Statistics*, 1923, p. 235.

[2] *Ibid.*, p. 232.

[3] See e.g. *Inquiry into the Causes and Remedies of the late and present Scarcity and High Price of Provisions in a letter to the Rt. Hon. Earl Spencer, K.G. . . .* (pp. 39–47).

[4] The preface is dated 11th November, 1800.

[5] P. 32.

[6] *Observations on the Publication of Walter Boyd, Esq., M.P.* (1801), p. 21.

[7] *Refutation of Certain Misrepresentations relative to . . . Bank Notes*, p. 21ff.

IV

Henry Thornton, M.P., a partner in the Banking firm of Down, Thornton and Free, was the younger brother of Samuel Thornton, a Director of the Bank of England, and Governor from 1799–1801.[3] Called to give evidence before the Committees of 1797 he made several observations which suggested that his thoughtful and practical mind was already able to view in their true perspective the causes and effects of the Restriction. He called attention, for instance, to the danger of inflation inherent in the legal limitation of the rate of interest to 5 per cent., and to the fluctuations in the demand for notes due to changes in liquidity preference. These considerations, with many others, took their place in his comprehensive *Enquiry into the Nature and Effects of the Paper Credit of Great Britain* published in 1802, which was undoubtedly the clearest and sanest book which had up to then been written on monetary theory. It is still valuable, not only for its masterly summaries of contemporary commercial practice, but equally for its pregnant suggestions on theoretical issues. Unfortunately, its presentation is forbidding, largely because its structure is awkward and repetitive. Any summary of Thornton's contribution must therefore take drastic liberties with his arrangement. The following synopsis attempts to set out the main planks in his platform.

1. Adam Smith had held that paper money represented the value of the gold and silver of which it supplies the place. But this was to confuse *stock* and *flow*. The velocity of circulation of coin was usually more rapid than that of Bank Notes, and therefore a larger quantity of Notes than of coins would be required to effect similar transactions. Moreover, the velocity of circulation was not constant; in good times Bank Notes moved more rapidly than when confidence was impaired and there was a pressure for liquidity. Smith had also overlooked that Bank Notes were not the only substitute for coins; Bills of Exchange, and even Shares, were on occasion used for the purpose, though as these were interest-bearing paper they circulated more slowly than Bank Notes. [Thornton suggests, for instance, that on an average a Country Bank Note would effect a payment in three days, whereas a Bill of Exchange might take nine days.] And lastly, Smith had forgotten that methods of economising the supply of means of payment were constantly being invented, such for example as the Bank Clearing House.[4]

In this argument Thornton refers only incidentally to cheques. He is discussing the inevitability of credit linked up with the means of economising currency, and he mentions the practice of his time " merely by the transfer of the debts of one merchant to another in the books of the banker " to effect " a large proportion of what are termed cash payments . . . without the use of any bank paper ".[5] But he makes no attempt to allow for the effect either of cheques or of Bills of Exchange when discussing the relation between the circulating medium and prices. This failure is of very great importance; it is probably the most serious weakness in the whole book, and it is the more inexplicable since it was the common practice of all previous writers to consider, as he himself did, that bills, drafts, bank credits, etc., were a part of the circulation of money.[1] Thornton's omission in this regard is parallelled by the essential flaw in the 1844 Act, but it must be recalled that in 1802—and indeed right down to 1830—the part played by deposits in banking was much smaller than obtained in 1844.

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[1] " We owe much at present to the superstitious worship of Smith's name "—Francis Horner to Thomas Thomson, 15th August, 1803 (*Life* I, 229).

[2] " You will have *less* opportunity of being taught the elements of political economy here (London) . . . There are in London . . . no lectures whatever on political science." (Letter to L. Horner, 5th November, 1803—(*Life* I, 236.)

[3] Henry Thornton was, from the very start, confused with his brother. As Professor Hayek points out, McCulloch perpetuated the error in his *Literature of Political Economy* by styling the author of *Paper Credit* a Director of the Bank of England, which he neither was, nor (as a private banker) could be (*Paper Credit*, Hayek's edition, p. 15). The earliest occurrence of the misdescription which I have seen is in Howison, op. cit., p. 49 (1803).

[4] *Enquiry into . . . Paper Credit* (Hayek's edition), pp. 93ff.

[5] P. 101.

2. The second main doctrine upon which the Smithian Principle rested was that if notes were issued to excess some would be at once returned to the banks by those into whose hands they came, because they would be superfluous to their requirements. Thornton deployed against this view the whole forces of the Quantity Theory. The additional notes would compete with those previously circulating in order to purchase the limited supply of goods, and would therefore raise prices[2]; this would mean that extra notes *would* be required—in order to carry on the enlarged monetary turnover involved. But as long as the notes were convertible, the excess was self-correcting. The rise in prices would deter exports and encourage imports, affecting the balance of trade, and leading to a profit being obtainable by exporting the precious metals. Bullion would be withdrawn for this purpose, and the legal prohibition on melting down coins would be inadequate to prevent this also.[3] This link between the quantity of money, prices, and the exchanges was of the utmost importance to a clear conception of the dangers of credit creation. Here he uttered a warning that so far as prices were raised above the level which was consistent with " the general maintenance of our exchanges . . . it is the enlarged and too great quantity of notes of the Bank of England which is to be considered as the cause of the high price of goods . . ."[4] and not the reverse. At the same time he stressed that there was no such invariable sequence of cause and effect as to justify assuming that whenever gold was at a premium the circulation was necessarily excessive.

His analysis here clearly foreshadowed the distinction between internal and external " drains " which played so large a part in the Banking Theory.

3. Smith's argument that the discounting of real trade bills could not lead to an over-issue of paper had ignored the relationship between the current rate of interest and the rate of mercantile profit. Thornton developed here a *reductio ad absurdum* argument[5] which is worth quoting in some detail, since it disposed effectively of this main plank in the Smithian Theory. He contended (*i*) that the legal limit of 5 per cent. on the rate of interest charged was below the current rate of mercantile profit during periods of rising prices, such as the later stages of a War; (*ii*) that though an efflux of additional paper would temporarily discourage borrowing, yet as soon as prices had risen correspondingly it would be as profitable to borrow as before, since (*iii*) the rate of mercantile profit depended upon the volume of capital, which could not be rapidly increased. Therefore (*iv*) not even the largest issue of Bank Notes could produce any permanent diminution of the demand for them. Whence it followed (*v*) that a moderation on the part of borrowers could be obtained only by perpetual increases in the volume of paper.

This argument has been commended by Professor Hayek as the first clear foreshadowing of the concept of the Natural Rate of Interest,[6] and it undoubtedly led the way towards the abolition of the Usury Laws in 1833. It also influenced contemporary thought by directing attention to the double purpose of bank-issues, for conversion into capital and for purchases, which had been obscured by Smith's treatment of money as itself a part of capital.

The analysis in fact forged the double chain of reasoning which was to be the basis of the Bullion Report—that the quantity of money determined prices, in the sense that an increase in the note-issue raised prices, and a decrease lowered them; and that the level of prices in a country would, with a convertible currency, determine the quantity of money within it, by securing the export of any superfluous supply. But Thornton did not accept the view that the Bank of England solely controlled the quantity of money. Because of the constantly changing relationships between the circulation of Bank of England notes and those of country banks, he held in fact the view that there was little direct correspondence between the volume of Bank of England notes and the level of prices.[7] In any case an increase in Bank

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[1] Although he may perhaps have considered, as Wheatley explicitly argued (*Reflections on Currency and Commerce*, 1803, p. 176), that these media, being used only by merchants, had materially less influence on the general price level than Notes, which were generally used by the public.

[2] Pp. 235.

[3] P. 247.

[4] P. 221.

[5] P. 254ff.

[6] *Prices and Production*, p. 12.

[7] P. 224.

Notes might at times be an effect, not a cause, of a rise in prices, e.g. if trade was booming.

From these foundations he proceeded to build up a constructive policy for Bank action:

4. In the first place, " Gold coin is to be viewed chiefly as a standard by which all bills and paper-money should have their value regulated as exactly as possible ".[1] But a gold stock in the country was needed for three separate purposes : (*a*) To provide for fluctuations in the demand for coin; (*b*) to counteract the effects of an unfavourable balance of trade; and (*c*) to meet any extraordinary demand at home —though if this was due to panic it might exhaust any given reserve. For this reserve there could be no other possible depository than the Bank of England.

5. Secondly, there could be no absolutely rigid volume of notes which was always correct. For example, in times of crisis, more notes than usual would be demanded by those in pursuit of liquidity. Now a loss of confidence in the Bank might break it, because so long as it had any notes whatever in circulation it could not protect its gold reserve. Therefore its duty was to lend sufficient notes to keep up prices and preserve commercial confidence[2]; the additional quantity would not be returned upon it for bullion, because the velocity of their circulation would have slowed down. And the Bank of England had also a responsibility to support the country banks if and when runs were made on them.[3] This again implied that additional notes might have to be issued in a crisis, even though the exchanges were below par.

6. But subject to this, the volume of notes should be kept reasonably stable. being allowed to " vibrate " within certain limits, slowly and cautiously extended as trade expands, and diminished if gold was being exported and the exchanges were unfavourable.[4] Since the issues of the Country Banks were limited by their conventional convertibility into Bank of England Notes, these measures would go far to control the whole note circulation.[5] This view appears somewhat inconsistent with the disclaimer of the all-importance of the Bank of England issue which has been quoted above, but so long as the Bank of England was not blamed as solely responsible for any over-issue, Thornton was quite prepared to accept the fact of its influence being immensely important.

7. These principles would be appropriate whether Bank paper was or was not convertible, and if it was, they would suffice; since any surplus would then return to the Bank to be converted into gold for export. To this extent Thornton accepted Smith's analysis. But when the notes were inconvertible another criterion was necessary, and it was the principal contribution of Thornton's work that he showed clearly what this was: " It is the maintenance of our general exchanges, or, in other words, it is the agreement of the mint price with the bullion price of gold, which seems to be the true proof that the circulating paper is not depreciated."[6]

In thus championing the practical rule which the Bank had evolved for itself in its more enlightened days, Thornton re-directed attention from an internal to an external criterion—from the conditions under which notes were lent to the relative values of currencies. He destroyed the facile view that an adequate control over the note-issue was automatically exercised by the " real " demand from industry, substituting for it a watch over the exchanges.

Applying this rule to the conditions of his time, Thornton arrived at the last proposition which we need note:

8. The world price of goods (their gold price) would not be affected by any increase in the volume of paper in circulation in Great Britain, except to the extent by which the latter's monopoly could influence markets. Hence the effect of a too-extended paper credit in raising the prices of goods in this country could be measured by the difference between the market-price and the mint-price of gold or (which was nearly the same thing) by the extent of the fluctuations in the exchanges. This had never exceeded 12 per cent, which was accordingly the degree to which our paper was depreciated.[7] And this, believing as he did in the correctness of the Bank's action, Thornton attributed not to an excess of paper but to the unfavourable balance of trade induced by two successive bad harvests in this country.[8]

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[1] P. 111.

[2] P. 124.

[3] P. 181.

[4] P. 259.

[5] P. 219.

[6] P. 192.

[7] P. 261.

[8] P. 156.

Thornton was completely successful in demolishing the theory of banking expounded by his predecessors, but less so in constructing an alternative. His omission to consider deposits in current-accounts as substitutes for bank notes has already been mentioned, and it is of course clear that no bank policy could succeed which controlled the note-issue but left deposits unrestrained. We have also remarked his diffidence in attributing to Bank of England issues a decisive influence over the price-level, while yet believing that they constituted the really vital supply of purchasing-power and limited at the same time the issues of the Country Banks. This inconsistency introduced a hesitancy into the application of his theory which is remarkably absent from his clear and decisive analysis. His loyalty to the Bank of England led him to accept too readily the view that the premium on gold was due to a decline in trade due to bad harvests, though this was inconsistent with his own assertion that the rate of mercantile profit was rising. Nor did he recognise that his explanation of the necessity to suspend cash payments—a panic demand for gold at home superimposed upon an unfavourable balance of trade—was in itself inadequate to justify the continuance of the suspension. His theoretical demonstration was perfectly clear, and only his reliance on the practical good sense of the Bank can have prevented his applying it to the facts. It needs no demonstration that in his analysis there is to be found the whole substance of the Bullionist Theory.

V

Little was added, except trenchancy of exposition, by Thornton's immediate successors, Lord King and John Wheatley. The former provided one or two minor contributions, omitted or slurred by Thornton, to the demolition of the Smithian Principle—for instance, the point that the occasional convenience of a merchant seeking discounts had only a very remote connection with the permanent demands of the public for currency.[1] But while King attributed to the Bank a belief in the Smithian Principle, he conceded that actually no attempt was made to discount in full, only a variable proportion of the amounts applied for being allowed to each considerable merchant, according to his credit. In practice, as he pointed out, this meant that the volume of notes depended on no Principle at all, but merely upon the arbitrary decision of the Directors.[2]

As a constructive policy Lord King adopted Thornton's twin criteria; "A mixed consideration of the price of bullion and the state of the foreign exchanges"—though even this was not wholly satisfactory; actually there was *no* perfect standard available.[3]

Wheatley's contribution,[4] based on an exposure of the errors of mercantilism, shows an immense advance in organisation over the works of his predecessors, but makes no striking addition to the general argument. Two points are, however, worth noting. In the first place, he utilised the calculations of Sir G. Shuckburgh Evelyn, who had evolved an index number for the variations in general prices since A.D. 1050, to illustrate his thesis that paper money supplemented and not (as Smith had argued) replaced metallic currencies[5]; in the second place, he was sufficiently practical to abandon reference to the somewhat artificial price of gold as a criterion of the degree of inflation, and to concentrate on the foreign exchanges, remarking at the same time that this criterion would not preclude a universal depression of the value of money if all countries inflated together.[6] Wheatley's chief interest, however, was country banking, and further reference to his work may be deferred until the final section of this article.

VI

In 1804 the public was again interested in a currency problem, this time concerning Ireland.[7] Normally bills of exchange on London were sold in Dublin at a premium amounting to some 8 per cent. (the English shilling being worth thirteen Irish pence). In 1801 this premium was running at about 13 per cent., and for, a short time reached 16 per cent. On the 1st January, 1804, it was 17 per cent. or 18 per cent. In March, 1804, a Parliamentary Committee met to investigate the state of the Irish Circulation, and its Report, dated May 4th, 1804, attributed the increase quite definitely to over-issue by the Irish Banks, due in turn to the cessation of convertibility. The Committee suggested that the departure of gold had led to an excessive circulation of paper, over and above that needed to replace the gold withdrawn; firstly because the automatic check of a gold backing was abolished; secondly because forgeries multiplied, especially of the small notes; and thirdly because speculative trade and excessive discounts fed on and aggravated the over-issue, and were not damped down by the normal mechanism of gold losses, since this no longer operated, as it had done " in Fact as well as in Theory " before 1797.

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[1] *Thoughts on the Effects of the Bank Restrictions*, in *Speeches and Writings*, p. 68.

[2] *Ibid.*, p. 69.

[3] Pp. 71–72.

[4] *Remarks on Currency and Commerce*, 1803.

[5] *Ibid.*, p. 178.

[6] Pp. 186, 187.

[7] The conduct of the Irish Bank Directors had been strongly criticised in the previous year by, *inter alia*, Lord King (*op. cit.*, pp. 92–108).

The Committee sought (and to some extent manufactured) agreement between the height of the exchange premium and the size of the issues of the Bank of Ireland; and regarded the latter as responsible for concurrent increases in the issues of Country Banks. By way of remedy they proposed that Bank of Ireland Notes should be made convertible into Bank of England Notes, as a first step towards the restoration of their convertibility into coin. The immediate resumption of cash payments would have been preferable, but was, in the Committee's view, impracticable because of the expense and difficulties it would occasion to the Bank of Ireland. It was emphasised that the necessary reduction in the Bank's issues should be made cautiously.

All this, of course, represented an immense advance in, and clarification of, monetary theory since the discussions of 1797. The 1804 Committee enunciated with confidence and conviction principles which their predecessors had barely noticed in eliciting evidence, and had ignored altogether in their conclusions. In this respect the 1804 Report reflects clearly the influence of Thornton, who was a member of the Committee, and who obviously shaped many of the questions asked of witnesses. Some of these agreed with the opinions which the Committee adopted; mention may particularly be made of James Mansfield, an Edinburgh banker, who firmly attributed the exchange difficulties to over-issues.[1] As, however, neither he nor any other witness expressing what may now be called the " Bullionist " view did so more clearly than the Committee in its Report, it is unnecessary to quote them further.

On the other hand, several influential witnesses refused to accept the Committee's standpoint. For example, William Colville, Director of the Bank of Ireland, maintained " . . . my opinion is, that the circulation of Bank Paper in Ireland is in no shape the cause of the high Exchange; and it therefore seems incumbent upon me to point out what is the real and efficient Cause, which is, that Ireland owes more money than she is able to pay ".[2] (This " real and efficient Cause ", incidentally, was completely disproved by figures quoted in the Committee's Report, which showed that Ireland should have had a favourable balance of payments.)

Another anti-Bullionist witness was Jeremiah Harman, Director of the Bank of England for 33 years, who gave evidence to innumerable Committees and espoused a wholly " Smithian " view before all of them. On this occasion, driven into a corner by a masterly piece of Socratic interrogation, he admitted that excessive issues might result if Bank Rate was kept below the market rate[3]; but he evaded the obvious conclusion by describing notes so evoked as " forced issues ", as if that took them out of the plane of controversy. Immediately afterwards he resumed his normal line of argument by denying that a difference in values between gold and paper was any proof of depreciation in the latter.[4] Mr. Borrowes, a London merchant, not only refused to concede that the high exchange was due to an excessive issue of notes[5] but expressed the opinion that " the currency and exchange " are " two distinct things, and that the depreciation of the one, and the high Rate of the other may have distinct Remedies ".[6]

The attitude of the official representative of the Bank of England is particularly interesting. Mr. Winthrop, Deputy Governor (he became Governor the same year), agreed that if discounts were granted without limitation, even on safe bills, the currency might be increased beyond " the proper Quantity ".[7] In fact, the Bank of England did ration discounts.[1] But when asked the effect of an excess of paper currency upon the foreign exchanges, he replied, " I am not aware of any."[2] This remark lends special significance to the replies which he gave, in re-examination on the following day, to questions designed to elicit the criteria by which the Bank of England controlled its issue.

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[1] *Evidence*, 19.3.04 (p. 53).
[2] *Evidence*, 17.4.04 (p. 103).
[3] *Evidence*, 18.3.04 (p. 27).
[4] P. 28.
[5] *Evidence*, 6.3.04 (p. 7).
[6] P. 8.
[7] *Evidence*, 13.3.04 (p. 34).

> Previous to the Restriction, were not the Issues of Paper of the Bank of England in a great degree regulated by the Balance of Cash and Bullion in its Coffers ?—It ought always to be so in a great measure.
>
> Does not an unfavourable Exchange necessarily cause a drain on its Coffers of Specie to be remitted to the Continent ?—I think it generally does when there is no Restriction.
>
> Was not therefore the issue of the Paper of the Bank in a great degree limited by the state of the Foreign Exchange ?—As far as this appeared necessary, in the contemplation of the Directors of the Bank, it would certainly have its weight.[3]

It is a fair deduction from these answers that the Bank was not keeping abreast with the times. The link with the Foreign Exchanges was regarded as something incidental, reached only through the mechanism of specie movements; not as a direct index of the adequacy or superfluity of the volume of currency. Since specie movements could no longer affect it, the Bank seems to have thought that the Foreign Exchanges could be ignored. The concept of " the proper Quantity " of Bank paper, which Winthrop adduced, can be defined only in terms of the Smithian Principle, modified as necessary (in the empirical judgment of the Bank) to allow for the fact that it was lending to the Government as well as to the public. This seems to have been the working rule which the Bank had formulated for its own guidance. It is, however, an interesting fact that the Cash Reserve, which in February, 1799, had been £7½ mn. (34 per cent.), but by February, 1804, had fallen to £3.4 mn. (13 per cent.), the lowest point since 1797, rose again after the Committee had reported. By August, 1804, it was £5.9 mn. (22 per cent.) and by August, 1805, £7.6 mn. (25 per cent.), which level it retained, practically unaltered, for the next four years. It is interesting to speculate what might have happened had not the decline in the cash balance been arrested—conceivably by the publicity surrounding the Committee's discussions.[4]

VII

The final aspect of the contemporary development of monetary theory to which we should turn is the relationship between country banks and the Bank of England. Although representative quotation is difficult, because only one or two writers made systematic attempts to develop the question, and the remainder presented the most varied opinions, the increasing importance of this issue in relation to monetary policy makes it necessary to attempt a synthesis.

As early as 1797, the peculiar position of the Bank of England in relation to the Country Banks emerges from the celebrated reference by Sir Francis Baring to the Bank of England as the *dernier resort* to which all banks had recourse in an emergency.[5] To this, the third Report of that year added the view that Bank of England notes tended to be substituted for Country Bank notes when Country Banks got into difficulties, and that the Bank of England by its restraint in issues could prevent the gap caused by discredited notes from being fully filled, so that over-issue was not perpetuated.

From this view there developed during the next few years the wider doctrine that the Country Banks' note issues could not be dangerous to monetary stability because of the limitations imposed by the Bank of England. Walter Boyd, for example, argued that prices could not primarily be affected by the issues of the Country Banks because the latter must always remember that their notes are payable on demand in specie or in notes of the Bank of England.[6] Boyd was, of course, endeavouring to pin upon the Bank of England the ultimate responsibility for the rise of prices. Nevertheless, his argument on this point was adopted by Thornton, who added the refinement that the Country Banks had also to take into account the relationship between prices in their own localities and prices in London.[1] The latter were determined by the Bank of England; if the former rose too high in relation to the latter, the Country Bank notes would be exchanged for Bank of England notes in order to make purchases in London instead of locally, and the Bank of England would instantly return them to the issuing banks for encashment.

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[1] *Evidence*, 13.3.04 (p. 34).

[2] P. 35.

[3] *Evidence*,14.3.04 (p.43). *Cf* replies to similar questions in 1797, cited in a previous article, ECONOMICA, February, 1941, pp. 48, 49.

[4] I have argued elsewhere (ECONOMICA, May, 1940, p. 177) that in resting content with a 25 per cent. ratio during 1805–09 the Bank may have been influenced by a tradition that the pre-War ratio of 40—50 per cent. (1786–1793) was unnecessarily high.

[5] *Observations on . . . the Bank of England*, p. 22.

[6] *Op. cit.*, p. 19.

Such an attitude may be said, by 1803, to be common form. It was adopted, for instance, by the authors of the Irish Report, which expressed the opinion that Country Banks in Ireland would have been unable to put out the excessive issues in which they had indulged but for the fact that the notes of the Bank of Ireland were also seriously over-issued.

Wheatley, however, stoutly maintained that the alleged control by the Central Bank was mythical. What determined the size of the English Country Banks' issues was not their convertibility into Bank of England Notes, but the confidence with which, in good times, they were received and circulated by the public.[2] So long as the public was prepared to accept and utilise these issues, they would remain in circulation and, by raising prices generally, tend to perpetuate the need for them. Thornton's theory of " reflux " was defective because it ignored this generally inflationary trend.

In support of this thesis, Wheatley referred to a change, effected since 1795, in the relationship between country banks and the London banks, by which the latter obtained rediscount facilities at the Bank of England for their country correspondents. " By this intermediate agency, the London bankers were capable of extending the loans of the bank [of England] to their country connections, and of giving them that support which had been refused to their [i.e. the Country Banks'] direct applications; they [i.e. the London banks] thus secured to themselves the power of offering a certain quantity of bank notes, or specie, upon any sudden aspersion on their [i.e. the Country Banks'] credit."[3]

This inherently probable development receives some confirmation from the somewhat different, but not inconsistent, account given a year earlier in *Considerations on the Propriety of the Bank of England resuming its Payment in Specie at the Period prescribed by 37 Geo. III*. The anonymous writer explains that originally Country Bankers had kept a balance in London Banks as a reserve, and also as some recompense to the latter for their trouble in acting as London correspondents. Of late years, however, the practice had grown up of the London Bank lending to the Country Bank at interest, and the writer observes that sometimes this had been done too confidingly.[4] Not only was this symptomatic of the increased importance of London as the financing centre of the country—a position from which it had, principally as an intermediary, displaced the agricultural districts—but it also implied paradoxically some increase in independence on the part of the provinces. Once the Country Bank had become valuable to a London Bank as a profit-earning agent of the latter, it could rely on the whole resources of the metropolis being rallied to its defence if it appeared to be getting into difficulties. The result was to increase the dangers of crises; for the attempts of the London Banks to stave off disaster from their country correspondents might well lead to an accumulation of unsound business which would ultimately sweep away simultaneously large numbers of banks.

How far this opinion was correct must be treated as an issue of fact. If Thornton was right, each local inflation would have been constantly nipped in the bud by the competition of cheaper commodities from outside the locality, followed up by an encashment of the surplus issues. If Wheatley and the anonymous author were right, the process would have been delayed in good times by the universality of the expansion of country issues, despite the eventual competition from areas without country banks (notably London, where by this time Bank of England notes ruled supreme). Neither side discussed the potentially inflationary influence of bank *deposits*—presumably because, even in London, and still more outside it, the volume of cheque transactions was still small. Unfortunately, the available statistics of country banks and their issues are purely conjectural and can establish neither contention.

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[1] *Op. cit.*, p. 208.

[2] *Op. cit.*, p. 186, *cf.* Howison, p. 16, " Under a spirit of adventure [the reflux] admits of exception . . "

[3] P. 219. The pronouns in the original are completely ambiguous, but the interpretation given in the text seems to fit best the general argument.

[4] P. 32 ff.

Wheatley, however, drew from his anticipations—and was, I believe, the first to enunciate—the moral, subsequently embodied in modified form in the 1844 Act, that the circulation of Country Bank notes should be prohibited.[1] He proposed that the Bank of England should replace the Country Bank issues by its own notes, until " by a gradual contraction, and a long course of favourable exchanges, it be capable of providing a sufficient quantity of specie to supply their place ".[2] In this passage he anticipated by twenty years or more one main aim of the Currency School.

But his forebodings were not heeded, and the place of the Country Banks in the evolution of monetary policy was treated for the time being as a subsidiary one. It was assumed that their activities were circumscribed by factors beyond their control, and although, as the years went by, the theory of this control became more and more subtle—just as the commercial organism grew more and more complex—attention was focused exclusively on the Bank of England as the policy-forming institution.

For the historian of monetary theory, 1804 marks the end of an epoch. The weapons of the Currency Theorists have been forged; and while for many years they remained, if not idle, at least powerless to overcome the entrenched conservatism of the Bank and its defenders, the protagonists of the next forty years had little to do except whet them to a finer edge in order to achieve ultimate victory.

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[1] P. 225.

[2] P. 228.

The Poverty of Historicism, I.

By Karl Popper

The scientific interest in social or political questions is only slightly younger than the interest in physics; and there were periods in antiquity (I have Aristotle's collection of constitutions in mind) when the social sciences might have appeared, to a methodologist, to have advanced further than the physical sciences. But, with Galileo and Newton, physics began to leave all other sciences far behind, and since the time of Pasteur, the Galileo of biology, the biological sciences can claim to have proceeded on a similar path. The social sciences, however, have not yet found their Galileo.

In these circumstances, scientists who work in one of the social sciences are greatly interested in problems of method, and much of their methodological discussion is conducted with an eye upon the methods of the more flourishing sciences, especially physics. It was, for instance, a conscious attempt to copy the experimental method of physics which led, in the generation of Wundt, to reforms in the method of psychology; and attempts at reforming the method of the social sciences on somewhat similar lines have been made repeatedly since the time of Mill. In psychology, these reforms have had some measure of success, in spite of a large number of disappointments. But in the social sciences—those attempts have produced hardly anything but disappointment; and in methodological debates the problem soon became prominent whether the methods of physics are at all applicable to the social sciences, and whether it is not rather our prejudiced belief that they are applicable which is responsible for their deplorable state.

This situation suggests a simple classification of the claims proffered by methodologists of the more backward sciences. In accordance with their attitude towards the applicability of physics, we can classify their methodological claims as pro-naturalistic and anti-naturalistic; claims are pro-naturalistic or " positive " if they favour an application of the methods of physics, anti-naturalistic or " negative " if they oppose such an application to the science in question.

Whether a methodologist proffers (*a*) pro-naturalistic, or (*b*) anti-naturalistic claims, or whether he proffers (*c*) a mixed answer, will depend, largely, on his views with regard to the character of the science under consideration, or in other words, on his views with regard to the peculiarities of the field to be investigated by this science. *But the proposed answer must also depend very largely on his views concerning the methods of physics.* I believe this latter point to be the more important of the two; and I think the principal mistakes of methodological discussions arise from certain fundamental misunderstandings of the methods of physics; more particularly, from misunderstandings of the logical form of its theories and the methods of testing them, and of the logical function of observation and experiment. My opinion that such misunderstandings are important will, I hope, be justified in the critical Part of this paper, where I shall try to show that certain methodological claims are based on a misunderstanding of the methods of physics. Parts A and B, however, are confined to developing the claims themselves.

These methodological claims which I wish to develop and then to criticise, and which I shall call the *claims of historicism*, are almost invariably found in any discussion of the method of the social sciences. They are frequently adopted without any attempt to examine them, and simply taken for granted. What I mean by " historicism " will be explained at length in this paper. It will be enough if I say here that I mean by " historicism " a methodology of the social sciences that emphasises their historical character and aims at *historical prediction.* Since I am convinced that this historicist methodology is, at bottom, responsible for the backwardness of the social sciences, my presentation of these claims is certainly not unbiased. But I have tried very hard to make a case in favour of historicism, in order to give point to my subsequent criticisms. I attempt to present historicism as a well considered and well connected structure of doctrines; and I have not hesitated to construct arguments which have never, to my knowledge, been brought forward by historicists themselves, whenever I found that I could thereby strengthen their position. In other words, I am building up a theory which has often been maintained in parts, but hardly ever in the fully developed form presented here. This is why I have deliberately chosen the somewhat unfamiliar name " historicism ". By introducing it I hope to avoid discussions on merely verbal questions; for example, whether a certain argument may be correctly described as a part of this methodology.

A. The Anti-Naturalistic Claims of Historicism

In strong opposition to methodological naturalism in the field of sociology, historicism claims that certain methods of physics cannot be applied to the social sciences, owing to the profound differences between sociology and physics. For, physical laws, or the so-called " laws of nature ", it tells us, are valid anywhere and always, the physical world being ruled by a system of physical uniformities invariable throughout space and time, while sociological laws, or the laws of social life, are, on the other hand, very variable. Although historicism admits that there are plenty of typical social circumstances whose regular occurrence can be observed, it denies that these regularities of social life have the everlasting character of the regularities of the physical world. For they vary in the course of time and in different cultures, and depend, consequently, on a particular *historical situation*. Thus one should not speak, for example, without further qualification, of the laws of economics, but only of the economic laws of the feudal period, or of the early industrial period, and so on; always mentioning the historical period where the laws in question are assumed to hold.

Historicism claims that the historical character of social laws makes it impossible for us to apply the majority of the methods of physics to sociology. These claims will be treated in turn, the most characteristic for historicism being those concerning generalisation, experiment, the complexity of social phenomena, the difficulties of exact prediction, and the significance of methodological essentialism.

(1) *Generalisation.* The possibility of generalisation and its success in the physical sciences rests, according to historicism, on the general uniformity of nature; upon the observation—better perhaps described as an assumption—that in similar circumstances similar events will happen. This principle, which is taken to be valid throughout space and time, is said to underlie the method of physics.

Historicism insists that this principle is necessarily useless in sociology. Similar circumstances only arise within a single historical period, and do not persist from one period to another. Hence there is no long-run uniformity in society on which our long-term generalisations can be based—that is, if we disregard such trivial regularities as the truism that human beings always live in groups, or that the supply of certain goods is limited, of others, like air, unlimited, and that only the former can have any market or exchange value.

A method which ignores this limitation and attempts a generalisation of social uniformities, must, according to historicism, implicitly assume that the regularities in question are everlasting. Thus, a methodologically naïve view—the view that the method of generalisation can be taken over from physics by the social sciences—must lead inevitably to a false and dangerously misleading sociological theory, viz. to the denial that society develops, or that social developments, if there are any, will affect the basic regularities of social life.

Historicists often emphasise that, behind such mistaken theories, there is usually an apologetic purpose; and indeed, the assumption of persistent sociological phenomena can easily be misused for such purposes. For in the first place, it may be argued that unpleasant or undesirable things must be accepted since they are determined by invariable laws of nature. For example, the " inexorable laws " of economics have been invoked to demonstrate the futility of statutory interference with the wage bargain. The second, broader, apologetic misuse of the assumption of persistence leads to a general feeling of inevitability, and thus to a readiness to endure the inevitable calmly and without criticism. What is now will be for ever, and attempts to influence the march of events, or even to evaluate it, are ridiculous: one does not argue against the laws of nature, and to try to overthrow them can only lead to disaster.

These, says the historicist, are the conservative, the apologetic, the even fatalistic arguments which unavoidably follow from methodological naturalism in sociology.

In opposition to them, the historicist maintains that social uniformities differ widely from those of the natural sciences. They change from one historical period to another, and *human* activity is the means of their alteration. For they are not laws of nature, but man-made, and although they may perhaps depend on *human* nature, this nature may have the power to alter and possibly to control them. Therefore things can be bettered or worsened; in any case, activity need not be futile.

These tendencies of historicism appeal to those who feel a call to be active, to interfere with things, especially with human affairs, who do not want to acquiesce in the existing state of affairs. The tendency towards activity and against complacency of any kind may be called *activism.* I shall say more about the relations of historicism to activism in Sections 17 and 18; but I may here quote the well-known remark of a famous historicist (Marx) because it strikingly expresses the "activist" attitude: "The philosophers have only *interpreted* the world in various ways; the point however is to *change* it."

(2) *Experiment.* Physics uses the method of experiment; that is it introduces artificial controls, artificial isolation, and thereby ensures the reproduction of similar conditions, and the consequent production of certain effects. This method is obviously based on the idea that where circumstances are similar, similar things will happen. The historicist claims that such a method is neither applicable nor useful in sociology. It would not be useful, he argues, even if it were applicable, for similar conditions only occur within the limits of a single period (cp. above); the outcome of any experiment would therefore be of very limited significance. Besides, artificial isolation would eliminate precisely those factors in sociology which are most important. Robinson Crusoe, for instance, and his isolated individual economy, can never be a valuable model of an economy whose problems arise precisely from the economic intercourse of individuals and groups.

It is further argued that interesting experiments are not possible. Large scale experiments in sociology are never experiments in the physical sense. They are not made to advance knowledge as such, but to achieve political success. They are not performed in a laboratory detached from the outside world; but rather, their very performance alters the conditions of society. They could never be repeated under precisely similar conditions, since conditions are altered by their very performance.

(3) *Novelty.* The argument we have just mentioned deserves elaboration. Historicism argues, as indicated, against the possibility of repeating large scale social experiments in precisely similar conditions, since the conditions of the second performance must be strongly influenced by the fact that the experiment has been performed before. This argument rests on the idea that society, like an organism, possesses a sort of memory of what we usually call its history.

We can speak, in biology, of the life-history of an organism since this organism is partially conditioned by past events. If such events are repeated, they lose, for the experiencing organism, their character of newness, and become tinged with habit. Yet this is precisely why the experience of the repeated event is *not* the same as the experience of the original event—why the experience of a repetition is *new*. Repetition of observed events can therefore correspond to the emergence of novel experiences in an observer. Since it forms new habits, repetition produces new, habitual conditions. The sum total of the conditions—internal and external—under which we repeat a certain experiment on one and the same organism cannot therefore be sufficiently similar for us to speak of a real repetition, since even an exact repetition of environmental conditions would be combined with new internal conditions in the organism: the organism learns by experience.

The same, according to historicism, holds true of society, since society too experiences; it too has its history. It may learn only slowly from the (partial) repetitions of its history, but it cannot be doubted that it does learn, in so far it is partially conditioned by its past. Traditions and traditional loyalties and resentments, trust and distrust, could not otherwise play their important rôle in social life. Real repetition must therefore be impossible in social history and novelty must accordingly emerge. History may repeat itself—but never on the same level, especially if the events concerned are of historical importance, and if they exert a lasting influence on society.

In the world described by physics nothing that is intrinsically new can ever really happen. A new engine may be invented, but we can always analyse it as a re-arrangement of elements which are anything but new. Newness in physics is merely the newness of arrangement or combination. Historicism claims that, in opposition to this, social newness, like biological newness, is an intrinsic sort of newness. It is real newness, irreducible to a novelty of arrangement, For, in social life, the same old factors in a new arrangement are never really the same old factors. Where nothing can repeat itself precisely, a real newness must always be emerging. This is held to be significant for the consideration of the development of new stages or periods of history, each of which differs intrinsically from any other.

Historicism claims that nothing is of greater moment than the emergence of a really new period ; this all-important feature of social life cannot be investigated along the lines we are accustomed to follow when we explain novelties in the realm of physics by regarding them as re-arrangements of familiar elements. Even if the ordinary methods of physics were applicable to society, they would never be applicable to its most important features: *its division into periods, and the emergence of novelty*. When we grasp the significance of social newness, we are forced to abandon the idea that the application of ordinary physical methods to the problems of sociology can aid us in penetrating the problems of social development.

There is yet another aspect of social newness. We have seen that every particular social happening, every single event in social life can be said, in a certain sense, to be new. It may be classified with other events, it may resemble those events in certain features, but it will always be unique in a very definite way. But this leads to a situation which is, as far as sociological explanation is concerned, markedly different from that of physics. It is conceivable that we may be able, by analysing social life, to discover and understand how, and why, this particular event came to happen —we may clearly understand its *causes and effects*, the forces which brought it about and its influence on other events. We may nevertheless find ourselves quite unable to formulate general laws which would serve as a description, in general terms, of such causal links. For it may be only the one particular sociological situation, and no other, which could be correctly explained by the particular forces that we have discovered; these forces may themselves be unique, they may very well emerge only in this particular social situation, and never again.

(4) *Complexity*. The methodological situation just sketched has further aspects, apart from the sociological rôle of certain unique personalities. One of these aspects is the complexity of social phenomena. In physics we are dealing with a subject-matter which is much less complicated; in spite of that, we further simplify matters artificially by the method of experimental isolation. This method is not applicable in sociology (cp. above), and we are therefore faced with a twofold complexity—a complexity arising out of the impossibility of artificial isolation. and a complexity due to the fact that social life is a natural phenomenon that presupposes the mental life of individuals, i.e., psychology, and that this itself presupposes biology, which again presupposes chemistry and physics. The fact that sociology comes last in this hierarchy of sciences plainly shows us the tremendous complexity of the factors involved in social life. Even if there were everlasting sociological uniformities as there are physical uniformities, we may very well be unable to find them owing to this tremendous twofold complexity. But if we cannot find such uniformities, then it is pointless to maintain that they nevertheless exist.

(5) *Inexactitude of Prediction*. It will be shown in the discussion of its positive claims that historicism is inclined to stress the importance of prediction as a task of science. (In this respect, I am, for one, in full agreement with it, even though I do not believe in historical prediction.) Yet historicism argues that social prediction must be very difficult, not only on account of the complexity of social material, but also on account of a peculiar complexity arising from the interconnection of predictions with predicted events. The idea that predictions may influence predicted events is a very old one. Oedipus, in the legend, killed his father whom he had never seen before, but this was the direct result of the prophecy which had caused his father to abandon him. Historicists have recently pointed to the relevance of such connections to the social sciences, thereby increasing the difficulty of making exact predictions and endangering their objectivity. They claim that the assumption that the social sciences could ever be so far developed as to permit the accurate forecasting of social events, leads to absurd consequences, and that it can therefore be refuted on purely logical grounds. For, if social calendars of this sort were constructed and became known, they would certainly cause actions which would upset their own predictions. Suppose it were, for instance, predicted that the price for shares would rise for three days and then fall. Plainly everyone connected with the market would sell on the third day, causing a fall of prices on that day, contrary to the prediction. The idea, in short, of an exact and reliable social calendar is self-contradictory: exact social predictions are an impossibility.

(6) *Objectivity and Valuation.* In stressing the difficulties of prediction in the social sciences, historicism proffers arguments which are based on an analysis of the influence of prediction on predicted events. But this influence, according to historicism, can lead under many circumstances, to important repercussions upon the predicting observer. Similar considerations play a part in physics, where every observation is based on an exchange of energy between the observer and the observed; this leads to the uncertainty, usually negligible, of physical predictions, known as the " principle of indeterminacy ". It seems tenable to maintain that this uncertainty is due to an interaction between the observed object and the observing subject, who both belong to the same physical system of action and interaction. As Bohr has pointed out, there are analogies to the physical situation in other sciences, especially in biology and psychology. But nowhere is it of greater moment that the scientist and his object belong to the same world than in the social sciences, where it leads (as has been shown) to an uncertainty of predictions, which is sometimes of great practical significance.

We are faced, in the social sciences, with a full and complicated interaction between observer and observed, between subject and object. The awareness of the existence of tendencies which might produce a future event, and, furthermore, the awareness that the prediction might itself exert an influence on events predicted is likely to have repercussions on the content of the prediction; and these repercussions might be of such a kind as gravely to impair the objectivity of predictions, i.e., of the results of research in the social sciences.

A prediction is a social happening which may interact with other social happenings, and among them with the one which it predicts. It may, as we have seen, help to precipitate this event, but it is easy to see that it may also influence it in other ways. It may, in an extreme case, even *cause* the happening it predicts, i.e., this happening might not have occurred at all if it had not been predicted. In another extreme case the prediction of an impending event may lead to its prevention, so that, if the social scientist had abstained from predicting it, he would thereby have caused the event in question. There are clearly many intermediate cases between these two extremes. The action of predicting something, and that of abstaining from prediction, might both have all sorts of consequences.

Now it is clear that social scientists must, in time, become aware of such possibilities. A social scientist may, for instance, predict something, foreseeing that his prediction will cause it to happen. Or he may deny that a certain event is to be expected, thereby preventing it from occurring. And he may, in both cases, proceed in accordance with the principle which, apparently, ensures the objectivity of science: to speak the truth, and nothing but the truth. But in spite of truth-speaking, we cannot say that he has observed scientific objectivity, for in making forecasts which forthcoming happenings fulfilled he may have influenced those happenings in the direction that he personally preferred.

The situation here described is somewhat idealised, the historicist may admit, but he will insist that it brings out sharply a point we find in almost every chapter of the social sciences. The inter-action of the social scientist's pronouncements and social life almost invariably creates situations in which we have not only to consider the truth of such pronouncements but also their actual influence on future developments. The social scientist may be striving to find the truth; but, at the same time, he always exerts a definite influence upon society. The very fact that his pronouncements *do* exert an influence destroys their objectivity.

We have, so far, assumed that the social scientist really strives to find the truth, and nothing but the truth; but the situation we have described makes plain, the historicist will point out, the difficulties of our assumption. For where interests and tendencies have such influence on the content of scientific theories and predictions, it must become most doubtful whether the extent of bias can be determined; and we need not be surprised to find that there is very little in the social sciences which resembles the objective and ideal quest for truth which we meet in physics. We must expect to find as many tendencies in the social sciences as can be found in social life; as many standpoints as there are interests. It may be questioned whether the historicist argument does not lead to that extreme form of relativism which holds that objectivity, and the ideal of truth, are altogether inapplicable in the social sciences, where only success—political success—can be decisive.

To illustrate these arguments the historicist may point out that, whenever there is a certain tendency inherent in a period of social development, we may expect to find sociological theories which influence this development. Social science may thus function as a midwife, helping to bring forth new social periods; but it can equally well serve, in the hands of conservative interests, to retard impending social revolutions.

Such a view may suggest the analysis and explanation of the different scientific views or schools and, above all, of the nature of their differences; either on the basis of tendencies and interests prevailing in a particular historical period (an attempt which has sometimes been called " historism ", and should not be confused with what I call " historicism "), or on the basis of political tendencies or class interests (an attempt which has sometimes been called the " Sociology of Knowledge ").

(7) *Holism.* Most historicists would maintain that there is yet a much deeper reason for the inapplicability of physical methods in the social sciences. Sociology, they would maintain, shares with all " biological " sciences, i.e., with all sciences that deal with living objects, the feature that it should not proceed in an atomistic, but in what is now called a " holistic " (or " universalistic ") manner. For the objects of sociology, social groups, must never be regarded as mere assemblages of persons. The social group is *more* than the mere sum total of its members, and it is also *more* than the mere sum total of the personal relationships holding between any two of its members. This is readily seen even in a simple group consisting of three members. If *A* and *B* are the founders of a group, its character will be different from a group consisting of the same members but founded by *B* and *C*. This may illustrate what is meant by saying that a group has a *history* of its own, and that its structure depends to a great extent on its history (cp. also section 3 on *Novelty*). A group can easily remain what it was even if it looses certain members of lesser importance. And it is even conceivable that a group may keep much of its original character even if *all* of its original members depart and are replaced by others. But the same members who now constitute the group might possibly have built up a very different group, if they had not entered the original group one by one, but founded a new one instead. The personalities of its members may have a great influence on the history and structure of the group, but this does not prevent the group from having a history and a structure of its own, and even from strongly influencing the personalities of its members.

All social groups have their own traditions, their own institutions, their own ritual. Historicism claims that we must study the history of the group, its traditions and institutions, if we wish to understand and explain it as it is now, and if we wish to understand, and perhaps to foresee, its future developments.

The holistic character of social groups, i.e., the fact that such groups are never fully explained as a mere assemblage of its members, throws light on the historicist's distinction between the novelty of physics, which merely involves new combinations or arrangements of elements and factors which are themselves not new, and the novelty of social life, which is real, and irreducible to a mere novelty of arrangement. For if social structures in general cannot be explained as combinations of their parts, or members, then clearly it must be impossible to explain *new* social structures by this method.

Historicism claims, on the other hand, that all physical structures can be explained as mere " constellations ", i.e., as the mere sum of their parts, together with their geometrical configuration. We may take the solar system as an instance: Although it may be interesting to study its history, and although this study may throw light on its present state, we know that, in a certain sense, this state is independent of the history of the system. The structure of the system, its future movements and developments, are fully determined by the present constellation of its members. Given the mutual positions, masses, and momenta, of its members at any one instant, the future movements of the system are all fully determined. We do not require additional knowledge as to which of the planets is older, or which was brought into the system from outside: the history of the structure, although it may be interesting, contributes nothing to our understanding of its behaviour, of its mechanism, and of its future development. It is obvious that a physical structure differs widely in this respect from any social structure, which cannot be understood, and whose future cannot be predicted, without a careful consideration of its history, even if we had complete knowledge of its momentary " constellation ".

Such considerations suggest strongly that there is a close connection between historicism and the so-called *biological or organic theory* of social structures, i.e., the theory which interprets social groups by their analogy to living organisms. Indeed holism is claimed to be characteristic of biological phenomena in general, and especially necessary in considering the history of various organisms as influencing their behaviour. The holistic arguments of historicism are thus liable to stress the similarity between social groups and organisms, although they need not necessarily lead to an acceptance of the biological theory of social structures. Similarly, the well-known theory of the existence of a *group-mind* or of a *group-spirit*, as the carrier of the *group-traditions*, although not necessarily itself a part of the historicist argument, is closely related to the holistic ideas sketched above.

(8) *Intuitive Understanding.* We have dealt so far mainly with certain characteristics of social life, such as periodisation, novelty, complexity and holism, which according to historicism, render certain methods of physics inapplicable to social sciences. A more historical method of approach is therefore considered necessary in social studies. It is part of the negative, i.e., anti-naturalistic claims of historicism that we must try to understand intuitively the history of the various social groups and this claim is sometimes developed into a methodological doctrine which is very closely related to, but not invariably combined with historicism.

This doctrine maintains that the method of the social sciences, as opposed to the method of the natural sciences, is based upon an intimate understanding of social phenomena. The following oppositions and contrasts are usually stressed in connection with this doctrine: physics aims at causal explanation; sociology at an understanding of purpose and meaning. In physics events are explained rigidly, in quantitative terms, and with the aid of mathematical formulæ. Sociology tries to understand historical developments in terms of a more qualitative character, e.g., in terms of conflicting tendencies and aims. This is why physics operates with inductive generalisations whilst sociology can only operate with the help of sympathetic imagination. And it is also the reason why physics can arrive at universally valid uniformities, and explain particular events as instances of such uniformities, whilst sociology must be content with the intuitive understanding of the individual events, and of the rôle they play in particular complexes within particular struggles of interests, tendencies, and destinies.

I propose to distinguish between three different variations of the doctrine of intuitive understanding. The first maintains that a social event is understood when analysed in terms of the forces that brought it about, i.e., when the individuals and groups involved, their purposes or interests, and the power they can dispose of, are known. The actions of individuals or groups are here understood as being in accordance with their aims—as promoting their real advantage or, at least, their imagined advantage. The method of sociology is here thought of as an imaginative reconstruction of either rational or irrational activities, directed towards certain ends.

The second variation goes further. It admits that such an analysis is necessary. particularly in regard to the understanding of individual actions or group activities, But it maintains that more is needed for understanding social life. If we want to understand the meaning of a social event, a certain political action for instance, then it is not enough to understand, teleologically, how and why it was brought about. Over and above that, we must understand its meaning, the significance of its occurrence. What is here meant by " meaning " and " significance " ? From the standpoint I am describing as the second variation, the reply would be : A social event, e.g., a political action, not only exerts certain influences, it not only leads, in time, to other events, but its very coming into existence changes the situational value of a wide range of other events. It creates a new situation, demanding a re-orientation and re-interpretation of all objects and of all actions in that particular field. The creation of a new army, for example, by a certain country is an event requiring for its understanding the analysis of intentions, interests, etc. But we cannot fully understand the meaning or significance of this action without analysing also its situational value; the military preparations of another country, for example, which were fully sufficient for its protection up to that time, may now appear to be of little moment. In short, before any further factual changes have occurred, the whole social situation may have altered. We have thus, in order to understand social life, to go beyond the analysis of causes and effects, i.e., motives, interests, and reactions caused by actions—we have to understand all events as playing a certain rôle within the whole; they have significance in influencing the whole, but their significance is likewise determined by the whole.

The third variation of the doctrine of intuitive understanding goes even further than the second, while fully admitting everything maintained by the first and second variation. It holds, that to understand the meaning or significance of a social event, more is required than an analysis of its genesis, effects, and situational value. Over and above such an analysis, it is held necessary to analyse objective, immanent historical trends and tendencies, and to analyse the contribution of the event in question towards the historical process by which such trends become manifest. A full understanding of the Dreyfus Affair demands, for instance, over and above an analysis of its genesis, effects, and situational value, an insight into the fact that it was the manifestation of the contest between two historical tendencies in the development of the French Republic, democratic and autocratic, progressive and reactionary.

This third variation of the method of intuitive understanding, with its emphasis on historical trends or tendencies, suggests to a certain extent the application of *inference by analogy* from one historical period to another. For though it fully realises that historical periods are intrinsically different, and that no event can really repeat itself in another period of social development, it can admit that analogous tendencies may become dominant in different periods, which are possibly very far removed from one another. Such similarities or analogies have been said to hold, for instance, between Greece before Alexander, and Southern Germany before Bismarck. The method of intuitive understanding suggests, in such cases, that we should evaluate the meaning of certain events by comparing them with analogous events in previous periods, and thus assist ourselves to forecast new developments. It is clear, however, that any differences between two periods must be duly considered.

We see accordingly, that the method of understanding the meaning of social events goes far beyond a causal explanation. It is, in its character, holistic; it tries to determine the rôle played by the event within a structure; within a whole which comprises not only simultaneous parts but also the successive stages of a temporal development. Following the analogy of the idea of a mind or spirit of the group which was mentioned above in connection with holism, the third variation of the method of intuitive understanding might sponsor the idea of a mind or spirit of the age, as the bearer of the tendencies which play such a rôle in determining the meaning of sociological events.

But the method of intuitive understanding is not only appropriate to holism: it also fits very well the historicist emphasis on novelty. It will be seen, furthermore, in the discussion of the positive claims of historicism, that there is a very close connection between these claims and the third variation of the method of intuitive understanding, with its emphasis on historical tendencies.

(9) *Quantitative Methods*. Among the oppositions and contrasts usually stressed in connection with the doctrine of intuitive understanding (cp. section 8) the following has frequently been emphasised by historicists. In physics, it is said, events are explained rigidly, in quantitative terms, and with the aid of mathematical formulæ. Sociology, on the other hand, tries to understand historical development in terms of more qualitative character, e.g., in terms of conflicting tendencies and aims.

The argument against the applicability of quantitative and mathematical methods is by no means peculiar to historicists; and, indeed, such methods are sometimes even favoured by writers with strong historicists views. But some of the most persuasive arguments against quantitative and mathematical methods represent very well the point of view which I call historicism, and these arguments will be discussed here.

If we consider these anti-quantitative and anti-mathematical positions, a strong objection occurs to us immediately: the positions seem to be in conflict with the facts, since quantitative and mathematical methods have been used with great success in the social sciences. How then can their applicability be denied?

Against such an objection, and in defence of the anti-quantitative and anti-mathematical point of view, we may note some arguments which are typical for historicist ways of thought.

I am, so the historicist may say, prepared to grant the majority of what has been stated; but there still remains a tremendous difference between the statistical methods of the social sciences and the quantitative-mathematical methods of physics: The social sciences know nothing which can be compared to the *mathematically formulated causal laws of physics.*

Consider, for instance, the physical law that (for light of any given wave-length) the smaller the aperture of any hole through which a light ray passes, the greater is the angle of diffraction. A physical law of this type has the form: " Under certain conditions, if the magnitude *A* varies in a certain manner, then the magnitude *B* varies in a certain manner also ". In other words: such a law expresses a dependence of one measurable quantity on another and the manner in which the one quantity depends on the other is laid down in exact quantitative terms. Physics has been successful in expressing all its laws in this form. In order to achieve this, its first task was to quantify all physical qualities. For instance, it had to replace the qualitative description of a certain kind of light—e.g., a bright yellow-greenish light—by a quantitative description: light of a certain wave-length and of a certain intensity.

Such a process of quantification, of quantitatively describing physical qualities, is obviously a necessary pre-requisite for the quantitative formulation of causal physical laws. These enable us to explain why something has happened: under the assumption, e.g., of the law concerning the relations of width of aperture and the angle of diffraction, we can give a causal explanation of an increase of the angle of diffraction by the fact that the aperture of a hole was decreased.

Causal explanation, the historicist maintains, must also be attempted by the social sciences. They may, for instance, undertake to explain imperialism in terms of industrial expansion. But if we consider this example we see at once that it is hopeless to attempt quantification of sociological laws. For if we consider some such formulation as " the rate of territorial expansion increases with the degree of industrialisation " (a formulation which at least is intelligible, though probably *not* a true description of facts), we shall find very soon the absence of any available method which could quantify, or measure, the rate of expansion, or that of industrialisation.

To sum up the historicist argument against quantitative-mathematical methods, it is the sociologist's task to give a causal explanation of the changes undergone, in the course of history, by such sociological entities as, for instance, the forms of government. As there is no known way of quantifying such qualities, no quantitative laws can be formulated. Thus, the causal laws of the social sciences, supposing that there are any, must differ widely in character from those of physics, being not quantitative and mathematical, but qualitative. If they determine the degree of anything, they will do so only in very vague terms, and will permit, at the best, a very rough scaling.

It appears that qualities—whether physical or non-physical—can only be appraised by intuition. The arguments we have discussed here can therefore be used to support those in favour of the method of intuitive understanding.

(10) *Essentialism versus Nominalism.* These considerations of the qualitative character of sociological terminology lead further to the so-called *problem of universals*, one of the oldest and most fundamental problems of philosophy.

This problem, over which a major battle raged during the middle ages, is rooted in the philosophies of Plato and Aristotle. It is usually considered to be a purely metaphysical one; but it can, like most metaphysical problems, be so re-formulated as to become a problem of scientific method. We shall deal only with the methodological problem here, a brief outline of the metaphysical issue serving merely as an introduction.

Every science uses terms which are called universal terms, terms like " energy ", " velocity ", " carbon ", " whiteness ", " evolution ", " justice ", " state ", " humanity ". These are distinct from the sort of terms which we call singular terms or individual concepts, e.g., " Alexander the Great ", " Halley's Comet ", " The Great War ". Such terms as these are proper names, or labels attached by convention to the individual things denoted by them.

The nature of universal terms seems rather different. A hard, prolonged dispute arose between two parties, one holding that universals differ from proper names only in being conventionally attached to a *set* or *class* of single things and not to one single thing by itself. The universal term " white ", for instance, seemed to this party to be nothing but a label attached to a set of many different things, snowflakes, tablecloths, and swans, for instance. This is the doctrine of the *nominalist* party. It is opposed by a doctrine traditionally called *realism*, a somewhat misleading name, as is seen by the fact that this " realist " theory is sometimes also called " idealism "; I therefore propose to use for this anti-nominalistic so-called " realism " the name " essentialism ". Essentialists deny that we first collect a group of single things and then label them " white "; rather, they say, we call each single white thing " white " on account of a certain intrinsic property that they have in common—their " whiteness ". This property denoted by the universal term is regarded as an object which deserves investigation quite as much as the individual things themselves. (From the assertion that universal objects, for instance, whiteness, " really " exist over and above single things and sets or groups of single things, the name " realism " is derived.) Thus universal terms are held to denote universal objects just as singular terms denote individual things. These universal objects (called by Plato " Forms " or " Ideas ") designated by the universal terms are also called their " essences ".

But Essentialism not only believes in the existence of universals (i.e., of universal objects), it also stresses their importance. Singular objects, it points out, show many accidental features, features which are of no interest to science. To take an example from the social sciences: economics interests itself in money and credit, but it does not care about the particular forms in which coins, bank-notes, or cheques appear. Science must strip away the accidental and penetrate to the essence of things. But the essence of anything is always something universal.

These last remarks may serve to indicate some of the methodological implications of this metaphysical problem. Nevertheless, the methodological issue I am going to explain can be judged independently of the metaphysical issue; it arises from a somewhat different approach. The existence and differences of universal and singular objects need not be discussed, but only the ends and means of science.

The trend of thought which I propose to call *methodological essentialism*, introduced and advocated by Aristotle, holds that scientific research must penetrate into the essence of things, in order to be able to make them understandable, and to explain them. Methodological essentialists are inclined to formulate scientific questions in such terms as " what is matter ? " or " what is force ? " or " what is justice ? " and they believe that a penetrating answer to such questions, revealing the real or essential meaning of these terms, and thereby the real or true nature of the essences denoted by them, is at least a necessary pre-requisite of scientific research, if not its main task. As opposed to this, *methodological nominalists* would put their problems in such terms as " how does this piece of matter behave ?" or " how does it move in the presence of other bodies ? "; for methodological nominalism holds that the task of science is only to describe the behaviour of phenomena, and suggests that this is to be done by freely introducing new terms wherever necessary, and by re-defining old terms wherever convenient, to the utter neglect of their original meaning, words being merely regarded as useful instruments of description.

Most people will admit that methodological nominalism has been victorious in the natural sciences. Physics does not enquire, for instance, into the essence of atoms or of light but has used these terms with great freedom to describe certain physical observations, and also for certain important, extremely complicated structures of physical phenomena. So it is with biology. Philosophers might demand from biologists the solution of such problems as " what is life ? " or " what is evolution ? " and at times some biologists may feel inclined to meet such demands. Nevertheless, scientific biology deals on the whole with rather different problems, and adopts descriptive methods very similar to those used in physics.

In the social sciences, methodological naturalists can thus be expected to be favourably disposed towards nominalism, and anti-naturalists towards essentialism. But in point of fact essentialism seems to have the upper hand here; and it is not even faced by a very energetic opposition. The suggestion has therefore been made that *while the methods of the natural sciences are fundamentally nominalistic, social science must adopt a methodological essentialism.* It is argued that the task of social science is to understand and explain such sociological entities as the state; economic action; the social group; etc.; and that this can be done only by penetrating into their essences. Every important sociological entity presupposes universal terms for its description and it would be futile to introduce new terms freely in this task. The task of social science is to describe such entities clearly and properly, i.e., to distinguish the essential from the accidental; but this requires the penetration of their essence. Problems such as " what is the State ? " and " what is a citizen ? " (considered by Aristotle to be the basic problems of his *Politics*), or " what is credit ? " or " what is the essential difference between the churchman and the sectarian (or the church and a sect) ? " are not only perfectly legitimate, but are precisely the kind of question which sociological theories are designed to answer.

Although historicists may differ in their attitude towards the metaphysical issue, and in their opinion with regard to the methodology of natural science, it is clear that they will be inclined to side with the essentialists against nominalism so far as the methodology of social science is concerned. In fact, nearly every historicist I know of, takes this attitude. It is of interest to ask, however, whether it is only the general anti-naturalistic tendencies of historicism that are responsible for this, or whether there are any specific historicist arguments that may be urged in favour of methodological essentialism.

It is clear, first, that the argument against the use of quantitative methods in social science is applicable to this issue. The emphasis on the qualitative character of sociological terms, connected with the emphasis on causal explanation and intuitive understanding as opposed to mere description, indicates an attitude closely related to essentialism.

But there are other arguments which are more typical of historicism, since they follow the same trend of thought that is now familiar to us from previous sections.

(Incidentally, they are practically the same arguments which, according to Aristotle, led Plato to develop the first theory of essences.)

Historicism stresses the importance of change. Now in every change, the historicist might argue, there must be something that changes. Even if nothing remains unchanged, we must be able to identify what has changed in order to speak of change at all. This is comparatively easy in physics—in mechanics, for example, all changes are movements, i.e., spatio-temporal changes, of physical bodies. But sociology, which is chiefly interested in social institutions, is confronted with greater difficulties, for such institutions are not so readily identifiable after they have undergone change. In the simple descriptive sense it is not possible to regard a social institution *before* a change as the same as that institution *after* a change; it might, from the descriptive point of view, be entirely different. A naturalistic description of contemporary institutions of government in Britain, for example, might have to present it as entirely different from what it was four centuries ago. Yet we can say that, in so far as it is a *government*, it is *essentially* the same, even although it has changed. Its function within modern society is *essentially* analogous to the function it then fulfilled. Though hardly any describable features have remained the same, the *essential* identity of the institution is preserved, permitting us to regard one institution as a changed form of the other: we cannot speak, in the social sciences, of changes or developments without presupposing essence, and hence without proceeding in accordance with methodological essentialism.

It is plain, of course, that some sociological terms have been originally introduced in a purely nominalistic fashion, e.g., depression, inflation, deflation, etc. But even so they have not retained their nominalistic character. As conditions change, we soon find social scientists in disagreement as to whether certain phenomena are correctly described as real inflations or not. Thus precision demands investigation into the essential nature of inflation.

It can be said accordingly of any sociological entity that it " might, so far as its *essence* is concerned, be present at any other place and in any other form, and might likewise change whilst remaining in fact unchanged, or change otherwise than in the way in which it actually does " (Husserl). The thoroughness of possible changes cannot be limited *a priori*. It is impossible to say what sort of change it can withstand and remain the same. Phenomena which from some standpoints may be essentially different, may from others be essentially the same.

Accepting the historicist arguments developed above, it follows that a bare description of social developments is impossible; or rather, that a sociological description can never be merely a description in the nominalist sense. And if a sociological description cannot dispense with essences, a theory of social development will be much less able to do so. For who would deny that problems such as the determination and explanation of the characteristic features of a certain social period, together with its tensions and intrinsic tendencies and trends, must defy all attempts of nominalist methodologies ?

Methodological essentialism can accordingly be based on the historicist argument which actually led Plato to his metaphysical essentialism, the argument that change presupposes something that remains identical with itself—an essence. *History*, i.e., the description of change, and *essence* appear here as correlative concepts. But this correlation has yet another side: in a certain sense, essence also presupposes change, and in so far history. For if that principle of a thing which remains identical or unchanged when the thing changes is its essence (or idea, or form, or nature), then the changes which the thing undergoes bring to light different sides of aspects or possibilities of the thing and therefore of its essence. The essence, accordingly, can be interpreted as the sum or source of the potentialities inherent in the thing, and the changes (or movements) can be interpreted as the realisation or actualisation of the hidden potentialities of its essence. (This is a theory due to Aristotle.) It follows that a thing, i.e., its essence, can be identified only *through* changes: if we want to find out whether a certain thing is made of gold, we have, for instance, to weigh it, or to test it chemically, thus changing it and thereby unfolding some of its hidden potentialities. In a similar manner, the essence of a man—his personality—can only be known as it unfolds itself in his biography. Applying this to sociology we are led to the conclusion that the essence, i.e., the real character of a social group, can reveal itself and can be known, only through its history. But if social groups can be known only through their history, they can be determined only as historical concepts; and indeed, such sociological entities as the Japanese *state* or the Italian *nation* or the Aryan *race* can hardly be interpreted as anything else but concepts based on the study of history. The same is valid for social *classes*: the bourgeoisie, for instance, can be defined as the class that came to power through the industrial revolution, that pushed aside the landlords, and that is fighting and being fought by the proletariat, etc.

Essentialism may have been introduced on the ground that it enables us to detect an identity in things that change, but it furnishes in its turn the most powerful arguments in favour of a historical method of the social sciences, i.e., of historicism.

B. The Pro-Naturalistic Claims of Historicism

Although it is fundamentally anti-naturalistic, historicism is not willing entirely to abandon the notion of a common element in the methods of the physical and social sciences. For historicism thoroughly agrees with the generally accepted view (which I fully share) that sociology, like physics, is a branch of knowledge which has to be characterised as being at once *theoretical* and *empirical*.

By saying that it is a theoretical discipline we mean that sociology has to *explain and to predict* events, with the help of theories, or general laws which it tries to discover. By saying that it is empirical, we mean that it is backed by experience, that the events it explains and predicts are *observable* facts, and that *observation* is the basis for the acceptance or rejection of any propounded theory. When the success of physics is spoken of, we have in mind the success of its predictions: and the success of predictions can be said to be nothing but the empirical confirmation of the laws of physics. When we compare the success or lack of success of sociology with that of physics, then we assume that their success, if any, would be basically of the same kind, namely the confirmation of predictions. It follows that certain methods—prediction with the help of laws and the testing of laws by observation—must be common to physics and sociology.

I fully concur with this view, in spite of the fact that I consider it one of the basic assumptions of historicism. But I do not share the more detailed opinions which I shall develop in what follows, although they might appear at first sight to be fairly straightforward consequences of the general view just outlined. In fact, these opinions involve other assumptions, namely, the negative claims of historicism.

(11) *Large Scale Forecasts.* While insisting on the deficiencies of sociological predictions with respect to accuracy and precision, historicists hold that the scope and significance of such forecasts might compensate for these drawbacks. The deficiencies arise mainly from the complexity of social events and from the qualitative character of sociological terms. But although social science has in consequence a certain looseness, its qualitative terms at the same time provide it with a certain fullness and comprehensiveness of meaning. Examples of such terms are " solidarity ", " utility ", " prosperity ", etc. Predictions of the kind described, i.e., long-term predictions, the vagueness of which is balanced by their scope and significance, I propose to call " predictions on a large scale ". They are, according to historicism, the kind of predictions which sociology has to attempt.

It is certainly true that such large scale forecasts—distant forecasts of a wide scope but possibly somewhat vague—can be achieved in some sciences. Instances of important and fairly successful large scale prediction can be found within the field of astronomy. Examples are the prediction of sun spot activity on the basis of periodic laws (significant for weather conditions) or of daily and seasonal changes in the ionisation of the upper atmosphere (significant for wireless communication). These resemble eclipse predictions in so far as they deal with distant events, but they differ from them in being often merely statistical and in any case less exact in detail, timing, and other features. We see that large scale predictions are not perhaps impracticable in themselves; and if long-term forecasts are at all attainable by the social sciences then it is fairly clear that they can only be what we have described as large scale forecasts. On the other hand, it follows from our discussion of the negative claims of historicism that *short-term* predictions in the social sciences must suffer from great disadvantages. Lack of exactness must affect them considerably, for they can deal, by their very nature, only with details, with the smaller features of social life, since they are confined to brief periods. But a prediction of detail which is inexact in details is nearly valueless. Thus, if we are at all interested in social predictions, large scale forecasts remain, according to historicism, not only the most fascinating but actually the only forecasts worth attempting.

(12) *The Observational Basis.* A non-experimental observational basis is, in a certain sense of the word, always " historical " in character. That is so even with the observational basis of astronomy. The facts on which astronomy is based are contained in the records of the observatory; records which, for instance, inform us

that at such and such a date (hour, second) the planet Mercury has been observed by Mr. So-and-so in a certain position. In short, they give us a "register of events in order of time", or a chronicle of observations.

Similarly, the observational basis of sociology can be given only in the form of a chronicle of events, namely of political or social happenings. This chronicle of political and other important happenings in social life is what one customarily calls "history". History in this narrow sense, i.e., the history of political developments, is the basis of sociology.

It would be ridiculous to deny the importance of history in this narrow sense as an empirical basis for social science. But one of the characteristic claims of historicism closely associated with the denial of the applicability of the experimental method, is that history, political and social, is the *only* empirical source of sociology. Thus the historicist visualises sociology as a theoretical and empirical discipline, whose empirical basis is formed by a chronicle of the facts of history alone, and whose aim is to make forecasts, preferably large scale forecasts. Clearly, *these forecasts must also be of a historical character*, since their testing by experience, their verification or refutation, must be left to history, perhaps to future history. Thus the making and testing of historical forecasts, and ultimately of large scale historical forecasts, is the task of sociology as seen by historicism. In brief, the historicist claims that *sociology is theoretical history*.

(13) *Social Dynamics.* The analogy between social science and astronomy can be further developed. The part of astronomy which historicists usually consider, celestial mechanics, is based on dynamics, the theory of motions as determined by forces. Historicist writers have often insisted that sociology should be based in a similar way on a social dynamics, the theory of social movement as determined by social (or historical) forces.

Statics, the physicist knows, is only an abstraction from dynamics; it as, as it were, the theory of how and why, under certain circumstances nothing happens, i.e., why change does not occur; and it explains this by the equality of counteracting forces. Dynamics, on the other hand, deals with the general case, i.e., with forces equal or unequal, and might be described as the theory of how and why something does happen. Thus, only dynamics can give us the real, universally valid laws of mechanics; for nature is process: it moves, changes, develops—although sometimes only slowly, so that some developments might be difficult to observe.

The analogy of this view of dynamics to the historicist's view of sociology is obvious and needs no further comment. But, the historicist might claim, the analogy goes deeper. He might claim, for instance, that sociology, as conceived by historicism, is akin to dynamics because it is essentially a causal theory; for causal explanation in general is an explanation of how and why certain things happened. Basically, such an explanation must always have an historical element. If you ask someone who broke his leg how and why it happened, you expect and will probably get the history of the accident. But even on the level of theoretical thought, and especially on the level of theories permitting prediction, a historical analysis of the causes of an event is necessary. A typical example of the need for a historical causal analysis, the historicist will assert, is the problem of the origin or the essential causes that produce war.

In physics, such an analysis is achieved by a determination of the interacting forces, i.e., by dynamics; and the historicist claims that the same should be attempted in sociology. It has to analyse the forces which produce social change and create human history. From dynamics we learn how the interacting forces constitute new forces; and conversely, by analysing forces into their components, we are able to penetrate into the more fundamental causes of the events under consideration. Similarly, historicism demands the recognition of the fundamental importance of historical forces, whether spiritual or material; for example, religious or ethical ideas, or economic interests. To analyse, to disentangle this network of conflicting tendencies and forces and to penetrate to its roots, to the universal driving forces and laws of social change—this is the task of the social sciences, as seen by historicism. Only in this way can we develop a theoretical science on which to base those large scale forecasts whose confirmation would mean the success of social theory.

(14) *Historical Laws.* We have seen that sociology, to the historicist, is theoretical history. Its scientific forecasts must be based on laws, and since they are historical forecasts, forecasts of social change, they must be based on historical laws.

But historicism claims that the method of generalisation is inapplicable to social science, and that we must not assume uniformities of social life to be invariably valid through space and time, since they usually apply only to a certain cultural or historical period. Thus social laws—if there are any real social laws—must have a somewhat different structure from the ordinary uniformities based on generalisations. Real social laws would have to be " generally " valid. But this can only mean that they apply to the whole of human history, covering all its periods, and not only some of the single periods. On the other hand, there can be no social uniformities which hold good beyond single periods. Thus the only universally valid laws of society must be laws which *link up the different periods*. They must be *laws of historical development* which determine the transition from one period to another. This is what historicists mean by saying that the only real laws of sociology are historical laws.

(15) *Historical Prophecy versus Social Engineering*. As indicated, these historical laws (if they can be discovered) would permit the forecasting of even very distant events, although not with minute exactness of details. Thus the doctrine that real sociological laws are historical laws (a doctrine mainly derived from the limited validity of social uniformities) leads back, independently of any emulation of astronomy, to the idea of " large scale forecasts ". And it makes this idea more concrete, for it shows that these forecasts have the character of historical prophecies.

Sociology thus becomes, to the historicist, an attempt to solve the old problem of foretelling our future; not so much the future of the individual, but that of groups, and of the human race. It is the science of things to come, i.e., of impending developments. If the attempt to furnish us with political foresight of scientific validity were to succeed, then sociology would prove to be of greatest value to politicians, especially to those whose interests go beyond " our time ". Some historicists, it is true, are content to predict only the very next stages of the human pilgrimage, and even this in very cautious terms. But one idea is common to them all—that sociological study should help to reveal the political future, and that it could thereby become the foremost instrument of practical politics.

From the point of view of the pragmatic value of science, the significance of scientific predictions is clear enough. It has not always been realised, however, that two different kinds of prediction can be distinguished in science, and accordingly two different ways of being practical. We may predict (a) the coming of a typhoon, a prediction which may be of the greatest practical value because it may enable people to take shelter in time; but we may also predict (b) that for a shelter to stand up to a typhoon, it must be constructed in a certain way, for instance, of ferro-concrete.

These two kinds of predictions are obviously very different although both are important and fulfil age-old dreams. In the one case we are told about an event which we can do nothing to prevent. We shall call such a prediction a *prophecy*. Its practical value lies in our being warned of the predicted event, so that we can prepare to meet it (possibly with the help of predictions of the other kind).

Opposed to these are predictions of the second sort which we can describe as *technological* predictions since they form a basis of *engineering*. They are, so to speak, constructive, intimating the steps open to us *if* we want to achieve certain results. The greater part of physics (nearly the whole of it apart from astronomy and meteorology) makes predictions of such a form that, considered from a practical standpoint, they can be described as technological predictions. The distinction between these two sorts of predictions roughly coincides with the lesser or greater degree of importance played by experiment, as opposed to mere observation, in the science concerned. The typical experimental sciences are capable of technological predictions, while those employing mainly non-experimental observations produce prophecies.

I do not wish to be taken as implying that all sciences, or even all scientific predictions, are fundamentally practical—that they are nothing but either prophetic or technological predictions. I only want to draw attention to an existing distinction between the two kinds of predictions and the sciences corresponding to them. In choosing the terms " prophetic " and " technological ", I certainly wish to hint at a feature they exhibit if looked at from the pragmatic standpoint; but my use of this terminology is neither intended to mean that the pragmatic point of view is necessarily superior to any other, nor that that scientific interest is limited to pragmatically important prophecies and to predictions of a technological character. If we consider astronomy, for example, then we have to admit that its findings are mainly of theoretical interest even though they are not valueless from a pragmatic point of view; but as " prophecies " they are all akin to those of meteorology whose value for practical activities is quite obvious.

It is worth noting that this difference between the prophetic and the engineering character of sciences does not correspond to the difference between long-term and short-term predictions. Although most engineering predictions are short-term there are also long-term technological predictions, for instance, about the lifetime of an engine. Again, whereas most astronomical prophecies are long-term, most meteorological prophecies are comparatively short-term.

The difference between these two practical aims—prophesying and engineering—and the corresponding difference in the structure of relevant scientific theories, will be seen later to be one of the major points in our methodological analysis. For the moment it shall only be stressed that historicism, quite consistently with its negative claims as to experiments, pleads for historical prophecy, and against social engineering, as the practical aim of social science—the prophecy of social, political and institutional developments. The idea of social engineering, the planning and construction of institutions to suit certain purposes, appears to some historicists as possible, but either as outside the scope of social science, or falling under the superior sway of historical forces. Others regard it as impossible. They are the only consistent historicists, as the next section will show.

(16) *Sociology as the Theory of Historical Development.* These considerations have taken us to the very heart of the body of arguments which I propose to call " historicism ", and also afford the justification of the choice of this name. Social science is nothing but history—this is the thesis. Not, however, history in the traditional sense of a mere accumulation of historical facts. The kind of history with which historicists wish to identify sociology looks not only backwards to the past but also forwards to the future. It is study of the operative forces and, above all, of the laws of social development. Accordingly, it could be described as historical theory, or as theoretical history, since the only universally valid social laws have been identified as historical laws. They must be laws of process, of change, of development—not the pseudo-laws of apparent constancies or uniformities. For the historicists must try to get a general idea of the broad trends in accordance with which social structures change. But besides this, they should attempt an understanding of the causes of this process, the working of the forces responsible for change. They should try to formulate hypotheses of general trends underlying social development, in order that we might adjust ourselves to impending changes by deducing prophecies from these laws.

The historicist's notions of sociology can be further clarified by following up the distinction already made between the two different kinds of prognosis—and the related distinction between two classes of science. In opposition to the historicist methodology, we could conceive of a methodology which aims at a *technological social science*. Such a methodology would lead to the study of the general laws of social life with the aim of finding all those facts which would be indispensable as a basis for the work of everyone seeking to reform social institutions. There is no doubt that such facts exist. We know many Utopian systems, for instance, which are impracticable simply because they do not consider such facts sufficiently. The technological methodology we are considering would aim at furnishing means of avoiding such unrealistic constructions. It would be anti-historicist, but by no means anti-historical. Historical experience would be a very important source for its consideration. But, instead of seeking to discover laws of social development, it would look for the various laws pertaining to the constructing of social institutions or of uniformities such as the historicist denies to exist.

Apart from pertinent arguments previously discussed, the historicist could criticise the existence and utility of such a social technology in another way. Let us assume, he could say, that a social engineer has worked out a plan for a new social structure, backed by the kind of sociology envisaged. This plan we suppose to be both practical and realistic in the sense that it does not conflict with the known facts and laws of social life; and we even assume that the plan is backed by an equally practicable further plan for transforming society as it is at present into the new structure. Even so, historicist arguments can show that such a plan would deserve no serious consideration. It would still remain an unrealistic and Utopian dream, just because it does not take account of the laws of historical development. Social revolutions are not brought about by rational plans, but by social forces, for instance, by conflicts of interests. The old idea of a powerful philosopher-king who would put into practice some carefully thought out plans was a feudal fairy-tale, the democratic equivalent of which is the superstition that enough people of good will may be persuaded by rational argument to planned action. History shows that the social reality is quite different. The course of historical development is never dominated by theoretical constructions, however excellent; although such schemes might, admittedly, exert some influence, along with many other elements of a more irrational nature. But even if such a plan coincides with the interests of powerful groups it will never be realised in the way in which it was conceived, in spite of the fact that the struggle for its realisation would then become a major factor in the historical process. The real outcome, however, will always be very different from the rational construction. It will always be the result of contesting forces. Furthermore, under no circumstances can it become a stable structure. All social engineering, no matter how much it prides itself on its realism and on its scientific background, is doomed to remain a Utopian dream.

So far, the historicist would continue, the argument has been directed against the practical possibility of social engineering backed by some theoretical social science, and not against such a science itself. It can easily be extended, however, so as to prove the impossibility of any theoretical social science of the technological kind. We have seen that practical engineering ventures must be doomed to failure on account of very important sociological facts and laws. But this implies not only that such a venture has no practical value but also that it is theoretically unsound, since it overlooks the only important social laws—the laws of development. The " science " upon which it was allegedly based must have missed these laws too, for otherwise it would never have furnished the basis for such unrealistic constructions. Any social science which does not imply the impossibility of rational social construction is entirely blind to the most important facts of social life, and must overlook the only social laws of real validity and of real importance. Social sciences seeking to provide a background for social engineering cannot, therefore, be true descriptions of social facts. They are impossible in themselves.

Besides this decisive criticism, there are other reasons for attacking technological sociologies, the historicist will claim. They neglect, for instance, such features of social development as the emergence of novelty. The idea that we can construct new social structures rationally on a scientific basis implies that we can bring about a new social period in exactly the way we have planned it. Yet if the plan is based on a science covering social facts, it cannot account for intrinsically new features, only for newness of arrangement (cp. section 3). But we know that a new period will bring its own intrinsic novelty—an argument which must render any detailed planning futile, and any science upon which it is grounded, untrue.

These historicist considerations can be applied to all social sciences, including economics. Economics, therefore, cannot give us any valuable information concerning social reform. Only a pseudo-economics can seek to offer a background for rational economic planning. Truly scientific economics can merely help us to unveil the driving forces of economic development through different historical periods. It may help us to foresee the outlines of future periods, but it cannot help us to develop and put into operation any detailed plan for any new period. What holds for the other social sciences must hold for economics. Its ultimate aim can only be " to lay bare the economic law of motion of human society " (Marx).

(17) *Interpreting vs. Planning Social Change.* The historicist view of social development does not imply fatalism nor need it lead to inactivity—quite the contrary. Most historicists have very marked tendencies towards " activism " (cp. section 1). Historicism fully recognises that our wishes and thoughts, our dreams and our reasoning, our tears and our knowledge, are all forces in the social development. It does not teach that nothing can be brought about; it only predicts that neither your dreams nor what your reason constructs will ever be brought about *according to plan.* Only such plans as fit in with the main current of history can be effective. We can now see exactly the sort of activity admitted by historicists to be reasonable. Only such activities are reasonable as fit in with and help along the impending changes. Midwifery is the only perfectly reasonable activity open to us, the only activity that can be based upon scientific foresight.

Although no scientific theory as such can directly encourage activity (it could only discourage certain activities as unrealistic), it can, by implication, give encouragement to those who feel that they ought to do something. Historicism definitely offers this kind of encouragement. It even gives human reason a certain part to play; for it is scientific reasoning, historicist social science, which alone can so determine the direction of any reasonable activity that it will coincide with the direction of the impending development.

Historical prophecy and the interpretation of history must thus become the basis of any thought-out social action. Consequently, interpretation of history must become the centre of historicist thought; and in point of fact, it has. All thoughts and all activities of historicists are centred in the interpretation of the past, and in the use for predicting the future.

Can historicism offer hope or encouragement to those who want to see a better world? Only those historicists who take an optimistic view of social development as being intrinsically " good " or " rational ", in the sense of tending towards a more reasonable state of affairs, are in a position to offer such hope. But their belief would necessarily rest on faith alone, since *reason is denied the rôle of bringing about a reasonable world.* In fact, influential historicist writers have optimistically foretold the coming of a realm of freedom, in which human affairs could be planned rationally. But even they teach that the transition from the realm of necessity which mankind must suffer at present to the realm of freedom and reason, cannot be brought about by reason, but only by cruel necessity, by the inexorable laws of development.

Those who desire an increased influence of reason in social life can only be advised by historicism to study and to interpret history, in order to discover the laws of its development. If such interpretation reveals that changes fitting in with their desire are impending, then the desire is a reasonable one, for it agrees with scientific prediction. If the development happens to tend in another direction, then the wish to make the world more reasonable turns out to be entirely unreasonable; to historicists it is then just a Utopian dream. Activism can be justified only so long as it acquiesces in impending changes and helps them along.

We have already seen that naturalistic methodology as it appears to historicism implies a definite sociological theory—the theory that society does not develop. We now find that historicist methodology implies a similar sociological theory: that society develops with inevitable necessity through predetermined stages.

" When a society has discovered the natural law that determines its own movement, even then it can neither overleap the natural phases of its evolution, nor shuffle them out of the world by a stroke of the pen. But this much it can do; it can shorten and lessen the birth-pangs." This formulation (due to Marx) excellently represents the historicist position. Although it teaches neither inactivity nor real fatalism, historicism teaches the futility of any attempt to alter impending changes, a peculiar variety of fatalism, so to speak, a fatalism in regard to the trends of developments. And although the " activist " appeal (quoted at the end of section 1, on Generalisation) not merely to intrepret the world, but actively to change it, may find much sympathy with historicists because of its emphasis on change and process, it is apparently in conflict with the most significant claims of historicism, if we mean by " world ", as historicists would, the developing human society with its inherent laws of motion. For we see now: The historicist can only *interpret* the social development and aid it in various ways; his point, however, is that *nobody can change* it.

(18) *Conclusion of the Analysis.* It might be felt that the last formulations deviate from my avowed intention of outlining the historicist position as sharply and as convincingly as possible before proceeding to criticise it. For these formulations try to show that the " activist " tendencies of most historicists are to some extent defeated by the outcome of the historicist analysis itself. This, it might be felt, implies the charge that historicism is inconsistent. And, it may be said, it is not fair to allow criticism and irony to creep into an exposition.

Such reproaches would be based upon misapprehension: only those who are fundamentally adherents of " activism " and who cherish historicism only secondarily, can feel that my remarks are critical. There will be many who feel in this way, and who were originally attracted to historicism on account of its " activist " leanings; but this cannot make any difference to the fact that to those who are primarily historicists, my remarks will appear not as a criticism of their historicist claims but only as a criticism of " activism "; not of all forms of activism, to be sure, but of one of its more extravagant forms. As compared with methodological naturalism, such historicists would say, historicism does encourage activity, because of its stress on change, process, motion. Yet it certainly cannot blindly countenance all kinds of activities as being reasonable from a scientific point of view. Many possible activities are unrealistic, and their failure can be foreseen by science. This leads us historicists to impose limitations on the scope of what we can admit to be useful activity; and an emphasis upon these limitations is necessary for any clear analysis of historicism. The two Marxian quotations (the one in section 1, on Generalisation, the other in the last section) do not contradict each other, but are complementary; although the first (and older one) taken by itself might perhaps appear as slightly too " activist ", its proper limitations are set out by the second; and should the first have appealed to over-radical activists and influenced them to join historicism, then the second will have shown them the proper limitations of activity, even though it may alienate their sympathies.

It seems to me for these reasons that my exposition is not unfair, but merely clears the ground. Nor do I think that another remark in the last section, to the effect that historicist optimism must rest on faith alone since reason is denied the rôle of bringing about a more reasonable world, is to be considered as a criticism of historicism. It may appear critical to those who are, in the first place, optimists or rationalists. But the true historicist will see in this analysis only a useful warning against the romantic and Utopian character of both optimism and pessimism, in their usual forms, and of rationalism too. He will insist that truly scientific historicism shall be independent of such elements; we simply have to submit to the existing laws of development, as we have to the law of gravity.

The historicist may even go further, He may add that the most reasonable attitude to adopt is to *adjust one's system of values in such a way as to conform with the impending changes*. If this is done one arrives at a form of optimism which can be justified, since any change will then necessarily be a change for the better, if judged by that standard of values.

Ideas of this kind have actually been held by some historicists, and have even been developed into a fairly connected and rather popular historicist moral theory: the morally good is the morally progressive, i.e., what is ahead of its time in conforming to such standards of conduct as will be adopted in the period to come.

This historicist moral theory, which could be described as " moral modernism " or " moral futurism " (it has its counterpart in an æsthetic modernism or futurism), accords well with the anti-conservative attitude of historicism; it can also be considered as a reply to certain questions concerning values (cp. section 6, on *Objectivity and Valuation*). Above all that, it can be looked upon as an indication that historicism, characterised by us as a methodology and seriously examined in this investigation only in so far as it is a methodology, might possibly be amplified and developed into a full-scale philosophical system. Or to put it in another way: it seems not improbable that historicist methodology might have originated as a feature of a general philosophical interpretation of the world. For there can be no doubt that methodologies are, from the standpoint of history although not of logic, merely the by-products of philosophical systems. These historicist philosophies I intend to examine elsewhere. But first it will be necessary to criticise the methodological claims of historicism which have been analysed here.

Dr. Rhodes' Analysis of the Distribution of Single Incomes in the United States

By GEORGE GARVY

THE purpose of this note is to raise some questions concerning the method suggested by Dr. Rhodes in his paper " The Distribution of Incomes in the United States ", published in the August issue of ECONOMICA.

Dr. Rhodes' method implies that the number of incomes in each income bracket, with the exception of the highest and the lowest brackets, is determined by two different " laws ": one described by a formula derived for a different income range and extrapolated for this particular bracket, and another approximated by a curve fitted to residual frequencies including the bracket considered. In other words, the proportion of incomes falling into a particular bracket is the sum of two frequencies. To disentangle them, Dr. Rhodes fitted a curve to the section of the empirical data where frequencies are supposed to be not composite (namely, the part of the " Tail " to which the original curve was fitted), and from the formula obtained he estimated what part of total frequencies in the composite part of the distribution is " ruled " by this first distribution law. A second formula was derived for residual frequencies of a certain number of income brackets, and a new set of residuals obtained by extrapolating the second " law of distribution " (the " Head ") beyond the lower limit of the income range for which it was derived. Though no third curve was fitted to these residuals " as the number of known values of x is small " (p. 231), nothing in Dr. Rhodes' method prevents us from refining the anatomic approach. In another distribution, we could obtain a " Neck " in addition to a " Tail " and a " Head ". The decomposition of the total frequencies thus is guided not by economic considerations but merely by the endeavour to obtain several partial distributions that can be described by a " simple " formula and to add up as nearly as possible to the original frequencies.

We venture to challenge the rationale of this method for economic analysis. It is sometimes very important to derive, from empirical income data, particular distributions for component parts of the population. One might be interested in analysing separately farm and non-farm incomes, or incomes derived mainly from property and those earned in gainful pursuits; in other words, to obtain partial distributions for more homogeneous components of the empirical distribution. Such an endeavour can obviously be successful only if considerable external information concerning the empirical data is available (if, for instance, the assumption can be made that practically all incomes below $3,000 are essentially earned incomes); but the inherent difficulties can hardly be solved by the proposed method of successive fitting of curves, a technique which is possibly attractive for the mathematician but has scarcely any economic meaning.

This leads us to the core of the problem: Why do we try to describe an empirical income distribution by a mathematical formula? Probably mainly for the following three reasons:

1. In order to compare distributions for different populations.
2. In order to compare changes over time in the same population.
3. In order to interpolate (or to rearrange in convenient brackets) a given distribution.

Dr. Rhodes' method has obviously no particular advantages for No. 3. In the first two cases also, we can make little use of it, for after we have torn Head and Tail apart (and are left, in addition, with the " Beak " which in the example studied includes not less than a fourth of the total population) in one distribution, we are not sure that when we repeat the same operation with another distribution (either for another population or for the same population at a different time point) we shall obtain a comparable " Head " and " Tail ". If we decompose a distribution, making certain assumptions based on economic reasoning in order to obtain aggregates with similar characteristics, we might be able to draw conclusions from comparisons of partial distributions so obtained. If our " Head ", " Tail ", and " Beak " are merely products of a mathematical operation, there is nothing to warrant that when we compare " Heads " of two distributions we shall deal with comparable parts of empirically given distributions; and a comparison of the parameters for different Heads (or Tails) will hardly be informative.

Even for the same distribution, a comparison of the characteristics of the results of the anatomic analysis cannot convey any useful information. What possible economic meaning is in the statement that " the fortunate few, 1 per cent., of the Head get incomes greater than $3.119. The corresponding fortunate group in the Tail have incomes more than $20.420 " (p. 232)? As a matter of fact, the " fortunate few of the Head " are not an economic group but merely an artificial creation of Dr. Rhodes. If we had fitted to the Tail another curve, or derived the formula for the Tail for a different income range, the " fortunate few of the Head " would have other characteristics. Since all formulas derived by Dr. Rhodes yield at certain values for x negative values for n and z, the author candidly states that " our formulæ A and B must not be supposed to operate below $x =$ \$500 " (p. 228). But why does the " law of distribution " stop operating only at the point where it starts yielding negative frequencies (i.e., when it becomes obvious that it is absurd), and not two or three brackets before?

It is, finally, very regretful that Dr. Rhodes proceeds to the analysis of the distribution of " single incomes " even without pointing out the particularities of the material to which he chose to apply his new method. Otherwise the reader would learn that the particular value of these data lies in the fact that they cover the lowest end of the income pyramid (lumped by Dr. Rhodes together with the amorphous " Beak "). They also differ from most of the other available income distributions in that they include imputed income. The upper " Tail " of the distribution, on which the whole analysis of Dr. Rhodes is based, is unfortunately the least reliable part of the entire estimate of the distribution of single incomes and is subject to serious criticism. On the contrary, the distribution of family incomes (of greater significance for economic analysis since it represents more than 80 per cent. of the total national income) which is based on a field survey (and is not pieced together from various sources as is the case for the estimate of the distribution of single incomes[1]) and which is available in the same table quoted by Dr. Rhodes as the source for his data did not attract his attention. If Dr. Rhodes had, for instance, derived similar parameters for the distribution of family incomes, and compared them with those obtained for single incomes, he would see how little knowledge can be gained by the method which he suggests.

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[1] See: National Resources Committee, *Consumer Incomes in the United States*, Washington, D.C., 1938, p. 67A.

Comments on Dr. Garvy's Note

By E. C. RHODES

As a student, working in the field of the Social Sciences, I am interested, *inter alia*, in Distribution, especially any records of actual income distributions. Admittedly, I am very interested in income distributions of homogenous groups, if and when I get a sight of them. My experience is that data relating to strictly homogeneous groups are rare. In practice, one has to be content with what material is available, and to work on it, realising its imperfections, but hoping that there will emerge from an analysis something of value.

Dr. Garvy is critical of the fact that I used the data relating to " single " incomes rather than those relating to " family " incomes. In fact, I did start with the family data, on his grounds, that they accounted for a considerable share of the national income, but later decided that other valid reasons (family versus individual) should prevail. Ultimately, it is a matter for argument whether one course is preferable to another, and so far opinion is equally divided (one against one). It must be admitted that my main interest was in the " new method " (Dr. Garvy's words), and from the point of view of illustration which distribution was used did not matter a great deal. It may be said, in defence, that the appearance of the critical graph on p. 227 (ECONOMICA, August, 1943) was typical of similar graphs for " family " incomes, and for certain British income data.

It has always seemed to me to be a remarkable thing that for higher incomes many distributions were to be described by the simple Pareto law. Equally remarkable, to me, was the fact that the word " higher " was a purely relative term. Why should not all incomes be brought within the scope of some comparatively simple description ? One of the main difficulties in the enunciation of such a description presumably has been due to the lack of data relating to the distribution of all incomes. In my gropings towards the *desideratum*, I find an alternative to the strict Pareto law for " higher " incomes, but that this apparently does not apply in the lower ranges. This perhaps is not of interest to some economists, but it may be to those economists and others, who also till the field of the Social Sciences. I know that there are economists, very distinguished economists, who deny that statistical analysis of data is helpful to them, but there are others who, becoming aware of a statistical simplification, will try to weave this fact into their theories. I cannot therefore agree with some of Dr. Garvy's comments.

There is another matter of interest which Dr. Garvy does not mention as a useful result of a mathematical description of statistical data. Smoothing data in this way sometimes emphasises their imperfections. The imperfections may be the main interest. In our case, it may be worth while trying to find out why discrepancies occur between the facts and the theoretical description. It is admitted that, in our case, this is a by-product of the main endeavour.

Book Reviews

Economic Fluctuations in the United States and the United Kingdom, 1918–22. League of Nations. George Allen & Unwin. 1942. 90 pp. 6s. $1.50.

These two studies are intended to be a contribution to thought on the problem of assuring the re-employment of men demobilised from armies and munition factories in peacetime occupations. The approach is primarily statistical; the essential statistics for both countries are assembled and analysed with the skill for which the League's publications have long been conspicuous. They lead up to the conclusion that "the major problem of the transition from war to peace economy proved itself to be not one of getting demobilised men and machines re-employed, but one of the cyclical effects of the perhaps unavoidably bumpy nature of post-war pent-up demand" (p. 79).

The problem then is one of dealing with "cyclical" effects; but what are "cyclical" effects? There were trade cycles before 1914, but the periodicity of the nineteenth century did not reappear in the inter-war years. "Cyclical" is to be interpreted as meaning not periodically recurrent but arising from the causes to which economists attribute the trade cycle. Which then of several possible meanings is to be assigned to it?

The sequence of events "cannot satisfactorily be described as merely the development of a business cycle" (p. 12). But "certain disproportions that developed during the war had prepared the ground for a post-war boom which", "put into action again the United States business cycle mechanism, known before the war", and "was followed in normal sequence by a cyclical downturn and depression".

The demands of war and Government expenditure, only partially offset by decreased civilian expenditure, combined with the deterioration of the means of supply, caused a rise of prices (p. 12). These conditions continued for a time after the end of hostilities, and the rise of prices was intensified by traders and consumers buying in anticipation of future needs, and by speculation in commodities, property and shares (p. 18).

"The rise of prices made for a decline in the amount of consumption goods which the earners of stable money incomes could buy," and "under such conditions, for aggregate demand not to drop, speculative demand must take up an ever increasing part of total output to fill the gap of shrinking regular demand. To induce speculators to do this, prices must continue to rise. Evidently this process can only go on for a short period of time. The decline in regular demand and the accumulation of stocks undermine the basis for speculation. There are limits to the amount of credit the banking system, however elastic, can put at the market's disposal for speculative or quasi-speculative purposes" (p. 19).

"It is clear that a market, carried away by speculation, must collapse as soon as prices stop rising. . . . When prices no longer rise, speculative demand falls away. But real incomes remain stable at a low level and hence regular demand does not increase. So the aggregate demand from both sources diminishes and the boom breaks" (p. 20).

Is not this idea of a "speculative demand" supplementing the "regular demand" misconceived? Speculation in a strict sense assumes the forms of forward purchases and sales, and the speculative purchasers do not necessarily take delivery at all; they either resell before the transaction matures, or settle it by the discharge of a difference, or continue it. The holding of goods in stock is a business quite distinct from a speculative purchase, and in the markets where the principal materials and intermediate products are dealt in, the traders who hold goods in stock or in process can and usually do cover themselves by forward sales, and so transfer the speculative risk to the forward buyer. That does not apply to the finished products forming the stocks of retailers and of the wholesale dealers who supply them. For them no hedging facilities are open, and they adjust their stocks to the actual demand as estimated from their sales. They may, it is true, order goods in excess of what the stock requirements corresponding to their existing sales would justify. But these orders only result in an actual increase in supplies so long as unemployed productive capacity is available. Once full employment is attained, as it was in 1920, this form of speculation fails to absorb goods and spends itself in raising prices. It pushes up the prices charged by producers and their increased incomes generate more "regular demand", so that, unless retail prices are promptly raised, there is an actually diminution of stocks.

The idea that regular demand is restricted, because some incomes are fixed in money, is a mistake. Incomes as a whole, including profits and wages, expand in proportion to production and the price level; the existence of fixed incomes affects not the total but the distribution of the total among consumers.

There was nothing inherent in the state of activity in 1920 which would necessarily bring it to an end, *so long as money was forthcoming* to pay the incomes generated by it.

The authors of the League's studies include in the factors bringing activity to an end the limits to which the creation of credit for speculation is subject, and they say (p. 25) " it is probable that the monetary policy [of the Federal Reserve Banks] had some effect towards checking the expansion, which, however, would have come to an end in any case." In England " there are no indications that in the process of deflation that set in in the spring of 1920 the policy of the Bank of England constituted a material factor—though the Bank was in favour of a policy of ' gradual deflation '. In fact the Banks did not pursue a stringent policy of credit restriction " (p. 64).

In England the Government (on whom the Currency and Bank Notes Act, 1914, had imposed the responsibility for the currency and the reserve requirements) had in December, 1919, placed a limit on the issue of currency notes. Appeals to the banks to ration credit failed to produce results, and, when they showed their power of replenishing their cash reserves by letting their Treasury bills run off, the Bank of England inflicted on the market the severest dose of dear money that had been known for half a century. Was it sheer coincidence that dear money was accompanied by the phenomena of contraction and depression which were intended to result from it and had resulted in the past?

If there was no evidence of a causal relation, there never had been evidence in the past, except the universal acceptance of the principle. Nor is there any trace of evidence of any other cause. The interpretation of events in the League of Nations studies is purely theoretical.

Undoubtedly a state of expanding demand might come to an end of itself. If traders obtained the means of increasing their stocks not through bank advances, but by raising capital (for example by reinvesting their swollen profits) that would be so. But there is no reason to suppose that that has ever happened either in 1920 or on any other occasion.

R. G. HAWTREY.

Trade Relations Between Free Market and Controlled Economics. Economic, Financial and Transit Department of the League of Nations. Geneva, 1943. 92 pp. 4s. 6d.

In this memorandum Professor Viner discusses the problems of trade relationships, after the war, if some countries subject their foreign trade to direct regulation, whilst others desire to avoid such controls, apart from tariffs (which he apparently accepts as a legitimate method of influencing the price mechanism).

In the first place Professor Viner analyses the reasons for the adoption of direct Government regulation of foreign trade, the mechanisms employed, and the results. Three main devices are recognised: i.e., exchange control, import quotas and direct governmental conduct of foreign trade—either through Government monopoly, or through Government-sponsored or tolerated cartels. This analysis, as one would expect from Professor Viner, is concise and convincing, although little new light is shed on the subject, and one would have welcomed fuller discussion of the means by which exchange control can most easily be abandoned. He concludes that, although a case can be made out for resort to such devices under conditions of world-wide depression, economic and financial instability and threat of war, these direct controls are injurious to world prosperity, and to international economic collaboration, and that their substantial elimination is a pre-requisite for the attainment of a peaceful and prosperous world. At the same time he rightly insists that the case against direct Government regulation is conclusive only on the assumption that " there is available as an alternative a smoothly-working competitive market process, not subject to substantial interference by private monopolies and not restricted by tariff barriers more oppressive to trade and more discriminatory than the Government monopolies themselves " ; whilst he admits that many countries appear to be too strongly committed to direct regulation of foreign trade to justify any expectation that it will be possible to secure anything like universal abandonment of this system after the war.

His recommendations are, therefore, mainly concerned with the possibility of promoting and facilitating a restriction of direct trade controls by means of a multilateral agreement. The detailed proposals made in this connection form, perhaps, the most valuable part of the report and deserve serious consideration. But it must not be overlooked that their success depends upon the prior establishment of genuine international collaboration and a general desire to promote international specialisation and exchange.

VERA ANSTEY.

Wartime Labour Developments in Australia. By O. DE R. FŒNANDER. Melbourne University Press and Oxford University Press. 1943. xxix + 177 pp. 17s. 6d.

This volume is a useful pendant to Mr. Fœnander's earlier and more general studies of the working of the Commonwealth arbitration system in Australia, serving to complete, as he puts it, his contribution to " the study of legal-economics on a historical basis ". It presents the facts relating to the problem of synthesising the activities of the Arbitration Court with the National Security (Industrial Peace) Regulations, the problem of adaptation to acute wartime scarcities of labour and the conditions under which so-called " wartime loadings " have been granted by the Court to certain groups of wage-earners. The book, which is thoroughly documented throughout, concludes with some suggestions for post-war re-adjustment, but on the whole it does not penetrate very deeply into fundamental issues.

ALLAN G. B. FISHER.

A Retrospective View of the Bank Charter Act of 1844

By P. Barrett Whale

Peel's Bank Act has always seemed to me exceptional in English legislation because it did not represent an attempt to deal piecemeal with the immediate practical problems, giving support in this direction, imposing restrictions in that, but gave effect to a clear-cut theory—the theory that banking ought to be separated from the control of the currency. It is true, of course, that the Act was occasioned by practical difficulties in the form of recurrent financial crises; but there were acute theoretical differences with respect to the diagnosis of these difficulties and the direction in which a remedy was to be found. Whilst these issues were still unsettled as far as public opinion was concerned, Peel became convinced that the truth lay with the Currency School, and the result was that the Bank's Charter was renewed on the terms dictated by this theory. It is hardly necessary to say in these pages that the fact that the Bank was allowed a certain fiduciary issue did *not* represent a compromise with the views of the Banking School. That a useful economy might be secured by substituting credit money for gold to a certain amount, and that this was compatible with a gold standard system functioning automatically in response to gold movements, had already been explained by Ricardo in the writings which foreshadowed the proposals of the Currency School. But misunderstanding on this point is not uncommon.

The Act is more generally assigned importance as being the foundation of the position of the Bank of England as our central bank. This view results from the very general tendency to identify central banking with the possession of the monopoly of the note issue ; it ignores the considerable measure of central banking responsibility which the Bank had assumed before 1844. Moreover, it is of interest to notice that the theory on which the Act was based really amounted to a denial of the need for central banking at all, if by central banking we understand the *discretionary* control of money and credit by a single authority—in modern times, monetary planning. For on the one hand it was intended that the control of currency should be purely automatic, on the other hand, no special responsibilities were to be attached to the Bank of England in its banking business Not even the centralisation of the note issue was necessary to the plan: Joplin had proposed a system of multiple issuers, each with a separate fixed fiduciary issue, which would perhaps have satisfied the requirements of the theory more completely than the one which respect for the *status quo* caused to be adopted. It is now recognised that under the influence of this theory the practice of the Bank showed a retrogression in its development as a central bank during the decades immediately following the passage of the Act.

Later, as a result of the post-1844 crises, the view that the responsibilities of the Bank began and ended with the observance of the rules governing the Issue Department was abandoned. The Bank became more conscious than ever before of its duty to regulate credit conditions, and new measures were developed to exercise control over other members of the system. But down to 1914 the Act under which the Bank operated remained unchanged. That this was possible—that the legal framework provided by the Act could still be found acceptable when its underlying principles were no longer accepted—is to be explained by the fact that the freedom which the Act left to the Bank in the conduct of its banking business included the freedom to conduct it as a central bank. And as a central bank, our Bank of England has enjoyed an advantage over its fellows in the absence of legal restrictions on the kind of business it may undertake.

The importance of the restrictions on note issues was reduced by the further development of the cheque system and, perhaps one should add, taking the period 1844 to 1914 as a whole, by ample gold supplies. But it was still important. In the first place, the limitation of the provincial issues and their gradual extinction increased the dependance of the other banks on the Bank of England for cash supplies, and this must have helped to make the Bank's control more immediate and effective. It must be remembered, however, that the 1826 Act had already deprived both the country banks and the Bank of England of the right to issue notes for less than £5.

The provisions with respect to provincial issues are also recognised to have had certain effects on banking organisation. To begin with there was a certain check to the tendency to concentration, not only because amalgamation meant in certain cases the extinction of issuing rights, but also because any system of quotas based on the existing position tends to prop up the competitively weaker firms. But in the longer run the fact that the provincial issues could not expand must have helped to tip the scales in favour of London branch banking without issues as against provincial banking with issues. In turn the consolidation of commercial banking into a comparatively small number of nation-wide branch systems directed from London has eventually facilitated central control.

The limitation of the fiduciary issue of the Bank itself imposed an indirect limitation on its general credit policy. In the early days of central banking, it may have been of some advantage to the authorities to be able to justify restrictive measures by reference to the position of the Bank under the Act. This appears to have been the view of the Bank directors in the 'fifties and 'sixties. Certainly it can be said as a matter of history, that although the Act did not prevent financial crises, it had the effect that the Bank met these crises with a much more substantal gold reserve than before; this gold was largely immobilised under the terms of the Act, but once it had been freed by the dispatch of the Treasury Letter, the strain was quickly relieved. As the responsibilities of the Bank came to be more clearly recognised both by the directors and the public, however, the need to take refuge thus behind the law became less; on the other hand, the legal restrictions entailed disadvantages. In particular internal cash drains appear to have often exercised an undue influence on the banking policy. These cash demands could only have been met in part, however, by the further issue of notes in denominations of £5 and upwards; we must again remind ourselves of the pre-1844 minimum denomination restriction.

Since 1928 this difficulty has been remedied both by the provisions for varying the fiduciary issue and by giving the Bank the right to issue £1 and 10/- notes. We have retained, however, the form of regulation introduced by the 1844 Act—the authorised fiduciary issue and the separation of the accounts of the Bank into the two departments. This has meant that at any given time with a certain quantity of gold available and a certain quantity of notes in actual circulation, there has been a certain quantity of notes left over to form the cash reserve of the Banking Department. (A temporary exception to this statement is to be found in the interlude in 1939, when the possibilities of altering the price of gold and of making transfers between the Bank and the Exchange Equalisation Account deprived the restrictions on the note issue of any definiteness.)

This particular mode of regulation has not been followed exactly, as far as I know, by any other country. It is true that for a time in the nineteenth century the English model exercised an influence on central bank legislation abroad to the extent that note issue regulations often embodied the principle of a fixed fiduciary issue (usually with some relaxing provision). In the cases of Japan and Norway, this provision stood alone: in other cases it was coupled with other provisions—e.g., the Reichsbank regulated both by a "steuerfreies Kontingent" and the "Drittel-deckung" principle, or the complicated rules governing the issues of the Swedish Riksbank. But in these cases there was no separation of departments in the central bank accounts. In other cases, in more recent years, we find the separation of departments adopted, but this is conjoined not with the fixed fiduciary issue principle but with a proportionate reserve requirement—Commonwealth Bank of Australia and Reserve Bank of India. This is an odd arrangement which does not secure the advantage of the English system (prior to 1932) that the reserve in the Banking Department shows the amount available for meeting either an external or an internal drain. For if the issue department were to issue notes to the full multiple of its gold (and or exchange) reserve, only a portion of those remaining in the Banking Department could be converted into gold. In practice this is not done; but then the Banking reserve fails to show the full extent to which notes can be made available. More generally the modern tendency, following the Federal Reserve Act of 1913, has been to adopt the proportionate reserve system, applied to deposits as well as notes, and without any separation of departments

In its practical operation the English system has not been so very different from the more generally adopted proportional reserve system. It is commonly said in comparison of the two systems that the English gives less scope for expansion on the basis of a certain available quantity of gold, but that it calls for less drastic contraction when gold is lost. Against the first point it is to be said that any amount of scope can be given for expansion under the method of fixing the fiduciary issue by fixing the amount at a sufficiently large figure. The only real point of difference here, as I think experience has shown, is that it is found necessary to alter the amount of the fixed fiduciary issue from time to time; absolute magnitudes cannot be fixed for such long periods as proportions. The second point in the comparison—the suggestion that under the proportional reserve system a loss of gold must lead to a multiple contraction of notes—appears to be based on the unfounded assumption that the central banks so regulated habitually work with their reserves at or near the minimum set by the law. Actually this is only exceptionally the case, and with reserves well above the minimum the ratio can be allowed to vary. It is not denied, of course, that regard is paid to changes in the ratio, especially when it falls near the minimum. But the Bank of England also has traditionally allowed its policy to be influenced by the reserve shown in its Banking Department. This ratio is affected by the same factors as the reserve ratio of a foreign central bank—changes in gold, in notes in the hands of the public and in deposit liabilities. The English ratio is more " sensitive " to changes in these factors because a demand for notes decreases the reserve instead of increasing the liabilities and because the reserve is related to deposit liabilities instead of to the larger total of deposits and notes. On the other hand, there is no legal minimum proportion which can only be infringed subject to penalties or special permission.

The general trend of this discussion has been to show that the Act of 1844 has worked satisfactorily because it did not work in the way designed. It will be recalled that the Macmillan Committee wished to sweep away the limitation on the fiduciary issue and the separation into two departments; further indeed to abolish any connexion between the gold stock, now regarded purely as an international reserve, and the internal note circulation. The logic of their thought would point to leaving both the note circulation and the size of the gold reserve to the discretion of the Bank; but rather as a concession to popular doubts they decided to recommend, on the one hand, the fixing of an (adjustable) limit to total note circulation and on the other the requirement of a gold reserve of not less than a certain absolute figure. No action had been taken on these recommendations before the situation was changed by the abandonment of the gold standard and the establishment a year later of the Exchange Equalisation Account. From its inception the latter served to divorce the international reserve in part from the internal credit base; with the outbreak of this war the process was completed by the transference of practically all the Bank's gold to the Equalisation Account. The limit to the fiduciary issue has thus become in effect a limit to the total possible issue, as recommended by the Macmillan Committee; moreover it has become a matter of routine to adjust this limit at intervals as required to make good the depletion of the reserve of notes in the Banking Department. The division of the return into two departments remains as a mere survival of form.

Fond as we are of familiar forms, it seems unlikely that the framework set up by the Act of 1844 will survive the period of post-war reconstruction. One possibility would be to adopt the suggestion which Professor Hayek made before the war and realise the fundamental intentions of the Currency School by applying the fixed fiduciary issue principle to current deposits (of the whole system) as well as to notes. Without entering into the merits of this proposal, it suffices to say here that the trend of opinion appears to be very definitely unfavourable to the idea of a purely automatic system and in favour of " monetary planning ", or of using monetary instruments freely as an adjunct to general economic planning. It is more probable therefore that the form of the Return will no longer be designed from the point of view of circumscribing the freedom of action of the Bank, but solely with the object of giving the public the information it needs in order to assess the current situation.

The Act of 1844 itself is a landmark in the progress towards banking publicity; indeed it might be held that the Weekly Return was its most important innovation. The 1928 Act brought an improved classification of the items and this process may be continued further. But on the other hand, since the establishment of the Exchange Equalisation Account, there has been an important decline in publicity with regard to the international assets of the central monetary authority, now the Treasury and the Bank of England taken together. Already before this the substitution of other international assets for gold—the " gold exchange " system in its various forms—had deprived the publicity with respect to gold holdings of much of its significance. The question is whether the obscuration of the international debtor-and-creditor position, so alien to the ideas of 1844, is to be accepted as a permanent change. It seems to me that this is the most important issue that has still to be decided in the field covered by Peel's Act.

The Joint Statement by Experts on the Establishment of an International Monetary Fund

By E. Victor Morgan

When the original British and American plans for an international clearing system were published a year ago,[1] it was apparent that, important as were the differences between them, their main objectives were the same, and it seemed reasonable to expect that agreement would be reached on a basis of compromise. This compromise is now before us in the shape of a joint statement agreed upon by experts representing not only Britain and America, but the other United Nations as well. The statement is issued as a white paper (1944, Cmd. 6519), together with some explanatory notes by British experts, showing how the new proposals are related to the original British draft. This agreement by experts marks a considerable step forward, but the plans now agreed will not be binding on the governments of the United Nations until they have been accepted at a formal conference.[2] This is intended to be the next step, and the scheme may, therefore, still undergo some modification before reaching its final form. The general principles involved have already been very fully discussed in connection with the original proposals, and the purpose of the present article is chiefly to examine the relationship of the new proposals to the old, and to discuss the significance of the changes which have been made.

One defect of the original drafts was their extreme brevity. We were given, as it were, the bare bones of the proposed institutions, and the reader had to cover them with hypothetical flesh before he could see what they really looked like. The process itself was tiresome, and it was not always possible to be sure that the right result had been reached at the end of it; in fact several points received widely different interpretations from different commentators. The present statement is even more condensed than its predecessors, and the accompanying explanatory notes really explain very little. The present writer believes that the " informed discussion ", which is the declared object of publication, would have been better informed and more helpful if the authors of the plan had furnished a general account of how they expected it to work, as well as a set of rules and conditions. However, this has not been done, so we must content ourselves with deducing from the rules as much as we can of the nature of the game.

The objects of the scheme have again been briefly stated, and with some significant changes. First on the list is now, " To promote international monetary co-operation through a permanent institution which provides the machinery for consultation on international monetary problems " (I.1)[3]. The British reference to providing a *quantum* of international currency, and the American one to facilitating the utilisation of war balances are, of course, no longer appropriate. In place of " To stabilise the foreign exchange rates of the currencies of the United Nations . . ." of the original American plan, we now have, " To promote exchange stability, to maintain orderly exchange arrangements among member countries, and to avoid competitive exchange depreciation " (I.4). As in the British proposals, emphasis is now laid on " giving members time to correct maladjustments in their balance of payments, without resorting to measures destructive of national or international prosperity " (I.3). Yet side by side with this we still have, from the original American plan, the phrase, " To shorten the period and lessen the degree of disequilibrium in the international balance of payments of member countries " (I.6). It does not seem at all easy to reconcile these two ideas, and this is one of several points where further explanation would have been helpful. Read as a whole, however, the new statement of objectives lays more stress on international co-operation, suggests desire for a greater elasticity in arrangements, and shows more concern for the internal conditions of member countries. In all these respects, it is an improvement.

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[1] *United States Proposal for a United and Associated Nations Stabilization Fund* (April 7th, 1943) and *Proposals for an International Clearing Union* (April 8th, 1943). For the sake of brevity, these will be called the American plan and the British plan respectively.

[2] Since this was written, the conference referred to has been called, and is now (July, 1944) in session. Unfortunately, however, the results of its work will not be available in time for comment in the present article.

[3] Figures in brackets, e.g. (I.1) refer to sections and paragraphs of the Statement.

In technique, the new plan follows closely the American proposals. There is to be set up an " International Monetary Fund " (we may note in passing that it is no longer called a stabilisation fund) and it is to start life with a good solid capital in gold and member currencies. The fund has been deliberately chosen in preference to the alternative of an international bank, and the British experts point out that the two " technical set ups " serve essentially the same purpose but, " It has proved easier to obtain agreement on the mechanism of the proposed Fund, which has the appearance of being closer to something which is already familiar ". If this is so the choice was probably wise. The British plan was theoretically the more elegant, but confidence and co-operation are so essential to the working of any such institution that it would have been fatal to start with something in which members could not feel entirely confident. The one important practical difference between the two methods is that a fund may always run out of the currency of one or more of its members, and so the rules must provide for the " rationing " of scarce currencies. The new proposals on this subject, of which more later, reduce the likelihood of rationing, but they cannot, in the nature of things, remove the possibility.

But, if the general constitution of the Fund follows the American plan, there have been important modifications. The total amount of quotas has been raised from the $5,000 million originally proposed, to $8,000 million, for the United Nations, which would be further increased to $10,000 million if the whole world eventually came into the scheme (II.1). The formula for the distribution of quotas is not mentioned, though the authors must have had some formula in mind, as Dr. Harry White, of the United States Treasury, is reported as having given approximate figures for the quotas of Great Britain, the U.S.A. and Russia.[1] Each member must now pay up the whole of its quota, instead of the 50 per cent. proposed in the American plan. There is an obligatory gold subscription of 25 per cent. of each member's quota or 10 per cent. of its holdings of gold and gold-convertible exchange, whichever is the smaller (II.3). On the basis of Dr. White's figures, the U.S.A. would pay 25 per cent. of its quota (about $625 million) in gold, while most other nations would presumably pay 10 per cent. of their holdings of gold or gold-convertible exchange. It is impossible to compare accurately the amount of the resulting gold subscriptions with those provided for in the original American plan, but it can be said that the Fund's total holdings would probably be larger under the present scheme, chiefly owing to the large American contribution. On the other hand, countries whose gold resources have fallen very low would be asked for a smaller contribution, and this might be an important factor in favour of the present proposals from their point of view. The balance of quotas is to be paid in " local funds ", though it is not stated whether this means currency, securities or, as in the American plan, a combination of the two. Since, however, nothing is said about the Fund dealing in the securities of members, it may be assumed that this part of the the subscription will be paid wholly in members' domestic currency.

As in the previous plans, it is not intended that the Fund should operate in the foreign exchange markets, and the present provision reads, " Member countries shall deal with the Fund only through their Treasury, Central Bank, Stabilisation Fund or other fiscal agencies " (III.1). An important new provision is that the Fund shall keep its account of a member's currency with the central bank of that member. It is to be hoped that the account would figure as a separate item in the returns of central banks, and thus not only the authorities, but also the public would have a detailed picture of how the Fund's resources were being used. This form of keeping the accounts does away with the need for a new international monetary unit, which will at least spare economists and politicians any further thought over its name.

Members of the Fund undertake to maintain agreed exchange rates between their own currency and those of other members, and to fix buying and selling prices for gold corresponding to the exchange value of their currencies (IX.1 and 2). It is also provided that, " So long as a member is entitled to buy another member's currency from the Fund in exchange for its own currency, it shall be prepared to buy its own currency from that member with that member's currency or with gold " (III 5).

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[1] *The Times*, April 24th, 1944.

Exceptions to this rule are the control of capital movements (V.1), scarce currencies (VI.2), and balances which have accumulated before the scheme comes into full operation (X.2). Furthermore, members bind themselves " not to impose restrictions on payments for current international transactions with other member countries " subject to the exceptions noted above—" or to engage in any discriminatory currency arrangements or multiple currency practices without the approval of the Fund ". These last two phrases are more familiar in America than in England. A " discriminatory arrangement " is apparently one by which exchange control is used to induce importers to buy in one market rather than another, as opposed to leaving them to be guided by prices alone. " Multiple currency practices " arise when a nation which is short of any particular currency arranges for sums due in that currency to be paid into special " blocked " accounts, which could be used only for purchases within the debtor country. As we saw in pre-war Europe, this practice leads to the quotation of " unofficial " rates of exchange, often widely divergent from nominal parity.

In its operations, the Fund is a good deal more restricted than in the original proposals. With two exceptions, it is confined to providing a member, on its own initiative, with the currency of another member (III.3). The conditions on which a member may buy currency have been somewhat simplified. The buying member must, of course, not have been suspended, the currency desired must not have been declared scarce, and the purchaser must declare that the currency is required, " for making payments which are consistent with the purposes of the Fund ". In such a case, a member is entitled to buy the currency of any other member provided that the Fund's holdings of the purchaser's currency (after being raised to 75 per cent. of its quota if it has fallen below that figure) has not increased by more than 25 per cent. of its quota within the past year, and provided that it does not amount to more than 200 per cent. of the member's quota in all (III.2). The Fund may waive any of these conditions at its discretion, but it has no obligation to do so. The provisions for gradually increasing pressure on a debtor as its deficit increased, contained in the British and to a lesser degree in the American plan, have now been abandoned.

At this point we may conveniently ask how the new plan compares with the old ones with regard to the size of the surplus or deficit which can be financed through the Fund.

Dr. White puts the British quota at $1,250 million, say £300 million. Let us suppose, which seems at present unlikely, that the full 25 per cent. (£75 million) was paid in gold. The Fund would then hold the remaining £225 million in an account at the Bank of England. This, apart from the sterling which it might buy for gold or borrow, would represent the limit to the surplus which the Fund could finance for us. Should we incur a deficit instead of a surplus, we could buy foreign currency in exchange for sterling to the extent of £375 million (bringing the Fund's total holding of sterling to £600 million), provided that not more than £75 million of this was purchased in a single year.

Under the American plan, assuming quotas to be determined on the same basis as used by Dr. White, Great Britain would have a quota of five-eighths of £300 million, i.e., £187.5 million. Assuming again that the maximum proportion (12½ per cent.) was paid in gold, the fund would hold a further 12½ per cent. in sterling and 25 per cent. in British securities (the two together amounting in round figures to £70 million). This would be comparable with the previous figure of £225 million, the maximum surplus which could be financed through the Fund, apart from any sterling which it might buy or borrow. Under the American plan, however, the Fund had a positive obligation to buy sterling with gold up to the 12½ per cent. originally contributed before resorting to rationing. Should we incur a deficit, we had a right, subject to certain conditions, to purchase foreign currencies until the Fund's holdings of sterling amounted to 200 per cent. of our quota (a power of purchase of roughly £350 million). It will thus be seen that the new proposals make considerably more allowance for surplus countries than did the original American plan but, in spite of the larger quotas, there is comparatively little extra allowance for debtor countries.

Under the British plan there was no limit to the surplus which could be financed through the Clearing Union. Great Britain's quota would have been approximately £950 million. We could have incurred a deficit up to half that amount almost without condition, and a further deficit (ultimately up to the quota itself) subject to more stringent conditions, though 75 per cent. of the quota would have been the normal limit. The comparison might have worked out differently for some other countries, owing to the different formulæ for the allocation of quotas, but it is certain that the total means of payment provided by the present Fund fall far short of those which would have been provided by the Clearing Union. This fact is freely admitted by the British experts, who explain that, " It has been argued that the present proposals are as large a commitment as it is prudent to ask in favour of an, as yet, untried institution ".

We have seen that the present proposals enable the Fund to start with substantially larger holdings of member currencies, and so considerably reduce the chances of a currency becoming scarce. Moreover, if scarcity does threaten, the Fund has power at its option either to borrow the currency in question or to buy it for gold, and no limit has been set to either of these transactions (III.4). In the American plan purchases for gold were limited to the amount of the member's initial gold contribution.

The possibility of the Fund having ultimately to resort to rationing is, therefore, a good deal more remote than under the American plan, but it does, nevertheless, exist and must be provided for. Should rationing become necessary, the Fund is to prepare a report setting forth the reasons for the scarcity and its recommendations for bringing it to an end (VI.1). It must also inform other members of the scarcity, and propose an equitable method of apportioning the limited supplies among applicants. Members then have complete freedom in allocating their supplies among their own subjects, and are absolved, temporarily, from their obligations not to impose exchange restrictions on current transactions with the country whose currency is scarce (VI.2).

It has been argued by some commentators that rationing would be a fatal defect, which would lead inevitably to the breakdown of the scheme.[1] Though no one can deny that rationing has its disadvantages and even dangers, this seems an extreme view. It would only follow if a number of currencies become scarce at once, or if restrictions imposed during scarcity were maintained after the scarcity had passed. Furthermore, it assumes that the creditor country, in spite of the recommendations of the Fund, will take no effective action to restore equilibrium. The probability of this depends on a complex of political and economic circumstances. In the past surplus countries have absorbed gold in enormous quantities without allowing it to produce any restoration of equilibrium, even though they had heavy unemployment. But the gradual absorption of gold is something very different from the drastic check to exports which would follow the rationing of a currency, and which would certainly cause exporting industries to bring strong pressure on their government. There would be a number of alternative policies, which would alleviate the scarcity; internal monetary expansion or a reduction of tariff barriers would, in most cases, be the most desirable methods, though each might have its difficulties. Alternatives would be long-term foreign lending or an increase in the exchange value of the scarce currency, the latter being made possible by the more elastic provisions of the present plan. It is surely not unreasonable to hope that a country would take effective action by one or other of these means.

This is certainly the view taken by the authors of the plan, who apparently regard the pressure thus brought to bear on surplus countries as a positive advantage. The proposals, never very stringent, in the original plans for inducing a creditor to bear its share in the process of adjustment, have now been dropped altogether, and the British experts explain that " They have been replaced in the new proposals by a different, but perhaps more far-reaching provision with the same object in view "— the rationing proposals. As we have seen, these provisions would not become operative until a very considerable surplus had been accumulated, but the knowledge of their existence might induce a country to modify its policy so as to steer clear of them, long before their application became imminent. It is to this and to the international co-operation which the authors so wisely stress, that we must look for our assurance that the creditor members will do their full share in any adjustments that are necessary.

Even so, however, one must admit that there is a possibility that members might evade the consequences of rationing by means of agreements inconsistent with the purposes of the fund. Hence the safeguards to which members commit themselves in the obligations already mentioned. A member's right to impose exchange restrictions on dealings in scarce currencies lasts only so long as the scarcity, and the embargo on " discriminatory arrangements " and " multiple currency practices " applies without exception. Presumably the penalty for breaking these undertakings would be the loss of the facilities offered by the Fund.

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[1] E.g., Oxford Institute of Statistics, Bulletin, Vol. 5, Supplement No. 5.

Perhaps the most important single difference between the present plan and its predecessors is on the question of changes in the value of member currencies. In the British plan a member might depreciate its exchange by 5 per cent. if it had a deficit with the Clearing Union of more than half its quota for two years; otherwise all changes required the approval of the Board. In the American plan, all changes without exception required the approval of the Board, and by no less than a four-fifths majority. Under the present proposals, any member may make a change of up to 10 per cent. after consulting the Fund, but without necessarily securing its approval (IV.4). Beyond that, changes require the approval of the Fund, but only by a simple majority (IV.2). Further, the Fund is given a most important direction in making its decisions, " The Fund shall approve a requested change in the par value of a member's currency if it is essential to correct a fundamental disequilibrium. In particular, the Fund shall not reject a requested change necessary to restore equilibrium because of domestic social or political policies of the country applying for a change. In considering a requested change, the Fund shall take into consideration the extreme uncertainties prevailing at the time the parities of member currencies were initially agreed upon " (IV.3).

To illustrate the implications of this, let us suppose that the British Government, pursuing its declared policy of full employment, reached that state at a level of prices and wages incompatible with the agreed value of sterling. To reduce prices and wages while maintaining full employment presents formidable difficulties, both political and economic, and the government might well decide that a reduction in the exchange value of sterling would be the easier course. The British Government would ask permission to devalue if the change was one of more than 10 per cent., and all the Fund would have to decide would be whether, *given the* (" *social or political* ") *policy which the British authorities were following*, equilibrium in our balance of payments could be restored with sterling at its existing value. If not, then the British Government would have the right to devalue.

In recent years we have thought of changes in exchange rates as being in a downward direction, but upward changes may take place on exactly the same conditions, and it is possible that a surplus country, faced with the possibility of rationing, may find a convenient remedy in some appreciation of its currency.

There is, of necessity, a dilemma between stability of exchange rates and stability of internal monetary conditions. The authors of the original plans chose stability in exchange rates, but there is a marked change of emphasis in the present proposals; while still retaining as strongly as possible the barriers against competitive exchange depreciation, they largely remove the obstacles from moderate changes in rates necessary to meet deep-seated causes of disequilibrium. Experience has shown that a moderate change in exchange rates is not infrequently the easiest way out of the dilemma, and the change is, therefore, welcome, especially as the quota arrangements are less elastic than those envisaged by supporters of the original British plan.

The position of capital movements in relation to the scheme and to exchange control is made rather more clear than in the original proposals. It is stated that "A member may not use the resources of the Fund to meet a large or sustained outflow of capital ", and the Fund may require the institution of controls to prevent this. This provision does not, however, prevent the finance through the Fund of movements which are " reasonable in amount " and consistent with the purposes of the Fund, nor does it prevent movements financed out of members' own holdings of gold and foreign exchange (V.1). In the next section it is provided that controls established for the regulation of capital movements shall not be used, " To restrict payments for current transactions, or to delay unduly the transfer of funds in settlement of commitments " (V.2). It would appear, therefore, that the control envisaged is one under which applicants for exchange would have to furnish evidence of the purpose for which it was required, but under which no " legitimate " applicants would be refused. In practice this might well involve centralising exchange dealings in the hands of the government or a small number of authorised agents.

Another aspect in which there have been significant changes is in the part which gold is designed to play in the scheme. In the British plan, gold would have had a fixed value in " bancor " and would have functioned alongside the new currency as an alternative means of international payment. In the American plan a proportion of the quotas was to be paid in gold, the Fund was empowered to buy and sell gold, and there was a system of small fees and fines payable in gold. Besides the provision for an initial gold subscription already mentioned, the present plan contains the following clauses;—any member buying another member's currency for gold is expected to do so through the Fund; a member may repurchase any part of the Fund's holding of its currency in exchange for gold; so long as a member's holdings of gold and gold-convertible exchange exceed its quota, half its net purchases of foreign exchange from the Fund must be paid for in gold; when a member's holdings of gold or gold convertible exchange have increased during a financial year, the Fund may require half that increase to be used to repurchase its currency from the Fund, provided that this does not reduce the Fund's total holdings of its currency below 75 per cent. of its quota; the Fund may purchase scarce currencies for gold without limit (III. 4, 6 & 7).

The Fund will accumulate gold under the first four of these provisions, and it is not empowered to sell it except, " with a view to preventing a particular member's currency from becoming scarce ". Any member which would normally have exported gold is bound to sell it to the Fund (though there is an exception in favour of newly-mined gold) and no member may import gold (other than newly-mined gold) except from the Fund, and then only when the Fund's holdings of its currency have been reduced to the point of impending scarcity. The British experts' comment on this is that " the Fund has some gold resources, which may be gradually increased with the intention that such gold in the hands of the Fund will be freely available in the interests of equilibrium ". The first part of this sentence seems an understatement; the Fund appears bound to add to its stock of gold, and might possibly do so very rapidly: the second part is decidedly ambiguous, and we shall try to elucidate it by an examination of how these clauses might be expected to work.

Suppose that Great Britain has a deficit on current account. So long as our gold and gold-convertible exchange exceeds our quota, we must finance half that deficit by the sale of gold; if we wish to finance a higher proportion in that way we may do so, but all such sales of gold must be made to the Fund. The effect on the Bank of England (and/or the Exchange Equalisation Account) would be partly a loss of gold and partly an increase in the deposits of the Fund. The more gold we chose to sell, the less would be the increase in the Fund's account, and vice versa. Obviously a loss of gold and an increase in the Fund's account are both symptoms of the same thing, an adverse balance, and from the point of view of domestic policy, they should be treated in just the same way.

Under an ordinary gold standard, one country's loss of gold is another's gain, and the country importing gold should allow it to produce an expansion of credit. But under the present system, the Fund has no power to sell until a currency threatens to become scarce. Instead of importing gold, therefore, creditor countries will see the Fund's deposits with their central banks declining. Obviously, too, a decline in the Fund's deposits should be treated, from the point of view of internal policy, in just the same way as would be an import of gold.

But the larger the Fund's holding of gold, the less its holding of member currencies, and sooner or later some of them will begin to grow scarce. It might seem as if these provisions were thus reducing the elasticity of the scheme, but this is not so, for as soon as a currency threatens to become scarce the Fund can sell gold, and the more gold it has to sell the longer will it be able to postpone rationing. This does not mean, however, that its power is in any way increased for, if members had not been compelled to sell gold to the Fund, they would have been able to export it for themselves, and would have had less need to call on the resources of the Fund.

If we consider capital movements rather than transactions on current account, we find that members are free to finance such movements out of their own gold holdings, but they must do this by selling the gold to the Fund. The borrower would presumably have a deficit, so the result will be to reduce, temporarily, the Fund's holdings of its currency, and increase its power of purchase abroad. Apart from differences of accounting the result would seem to be the same as would have been produced if gold had changed hands in the ordinary way. The one possible point of difference is that the clause compelling a member to finance half its deficit by the sale of gold, so long as its holdings exceed its quota, would be some assurance that a member did not use its gold to finance capital movements, while running up a large deficit with the Fund on current account.

If this interpretation is correct, it is very difficult to attach much meaning to the statement that the Fund's holdings of gold would be " freely available in the interests of equilibrium ", or indeed to see much advantage in these provisions as a whole. It is to be hoped that, before the proposals take a final form, some further clarification of these sections will be forthcoming.

Unlike the original American plan, the present one is not intended to deal with abnormal indebtedness arising out of the war. Hence the clauses forbidding exchange restrictions will not come into force until members are satisfied that they have the means of settling outstanding balances without unduly straining their resources with the Fund (XI.1). During the transition period, members may retain exchange restrictions, though they are pledged to remove them as soon as possible (X.2). Any balances which have accumulated before the removal of such restrictions are not covered by the obligation of members freely to buy foreign currencies in exchange for their own (III.5). Finally, the Fund may recommend the removal of restrictions at any time, and any restrictions remaining after three years are to be reviewed in consultation with the Fund (X.3).

The mere fact that experts representing so many nations and such different points of view should have agreed on these proposals is a notable thing. In spite of the strong superficial resemblance which the finished plans bear to the American draft, it is evident that there has been a great deal of give and take in the discussion, and the result may be regarded as, on the whole, a highly satisfactory compromise. If, as we may now expect, these proposals are accepted with only minor modifications by the governments concerned, they will be a marked improvement to the machinery for international payments. But machinery, however perfect, will give good and lasting service only if it is worked in suitable conditions, and if it is not overdriven. The breakdown in the immediate pre-war years was due at least as much to bad working conditions and to over-driving as to the weaknesses in the machinery itself, and plans must be made for these things too. The present proposals are far from solving all the problems of international monetary relations, but they are a long step in the right direction, and the way in which that step has been taken is promise of further and even longer ones to come.

The Poverty of Historicism, II.

A CRITICISM OF HISTORICIST METHODS.

By Karl Popper

(19) *Practical Aims of this Criticism.* Whether the true motive of scientific inquiry is the desire to know, that is to say, a purely theoretical or " idle " curiosity, or whether we should rather understand science as our servant in solving the practical problems that arise in the struggle for life, is a question that need not be decided here. It may be admitted that the protagonists of " pure " or " fundamental " research deserve every support in their fight against the narrow view, unfortunately again fashionable, that scientific research is justified only if it proves to be a sound investment.[1] But even the somewhat extreme position (to which I personally incline) that science is most significant as one of the greatest spiritual adventures that man has yet known may be combined with a recognition of the importance of practical problems and practical tests for the progress of science, whether applied or pure; for practice is invaluable both as a stimulus and as a check to scientific speculation. One certainly need not adopt a pragmatist philosophy in order to appreciate Kant's saying:[2] " To yield to every whim of curiosity, and to allow our passion for inquiry to be restrained by nothing but the limits of our ability, this shows an eagerness of mind not unbecoming to scholarship. But it is wisdom that has the merit of selecting, from among the innumerable problems offering, those whose solution is important to mankind."

The application of this to the biological and perhaps even more to the social sciences is clear. Pasteur's reform of the biological sciences was carried out under the stimulus of very practical problems, which were in part purely industrial and agricultural. And social research nowadays has a practical urgency surpassing even that of cancer research. As Professor Hayek says,[3] " economic analysis has never been the product of detached intellectual curiosity about the *why* of social phenomena, but of an intense urge to reconstruct a world which gives rise to profound dissatisfaction "; and some of the social sciences, other than economics, that have not yet adopted such an outlook, show by the barrenness of their results how urgently their speculations need practical checks.

The need for the stimulus of practical problems is equally clear when we consider inquiries into the methods of scientific research, and more especially, into *the methods of the generalising or theoretical social sciences* in which we are interested here. The more fruitful methodological debates are always inspired by certain practical problems which face the research worker; and nearly all methodological debates which are not so inspired are characterised by that atmosphere of futile subtlety which has brought methodology into disrepute with the practical research worker. It should be realised that methodological debates of the more practical kind are not only useful but also necessary. In the development and improvement of method, as of science itself, we learn only by trial and error, and we need the criticism of others in order to find out our mistakes; and this criticism is the more important as the introduction of new methods is usually a change of a more fundamental and revolutionary character than any other kind of progress. All this may be illustrated by examples such as the introduction of mathematical methods into economics, or of the so-called " subjective " or " psychological " methods into value theory. A more recent example is the combination of the methods of this theory with statistical methods (" demand analysis "). This last methodological revolution was in part the outcome of prolonged and largely critical methodological debates; a fact from which the apologist for the study of method may certainly draw encouragement.

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[1] The question is an old one. Even Plato sometimes attacks " pure " research. For its defence, see T. H. Huxley, *Science and Culture* (1882, p. 19f.), and M. Polanyi, Economica, N.S., Vol. VIII (1941), pp. 428ff. (In addition to the books there quoted, see also Veblen, *The Place of Science in Modern Civilisation*, pp. 7ff.)

[2] Kant, *Dreams of a Ghost Seer*, Part II, Ch. III (*Werke*, ed. E. Cassirer, Vol. II, p. 385).

[3] Cp. Economica, Vol. XIII (1933), p. 122.

A practical approach to both social science and its methodology is advocated by most of the followers of historicism who hope to transform social science with the help of the historicist method into a powerful instrument in the hands of the politician. It is this recognition of the practical task of a methodology of the social sciences which provides something like a common ground for a discussion between the historicists and some of their opponents; and it is on this common ground that I shall try to criticise historicism as being a *poor method*, unable to yield the results it promises.

(20) *The Technological Approach to the Social Sciences.* Although this paper deals with historicism, a methodology with which I disagree, rather than with those methods which, in my opinion, have been successful, and whose further and more conscious development I recommend, it will be useful briefly to characterise these latter methods first, in order to clarify the tendencies that underlie my criticism. For convenience, I shall label these methods as those of a "*piecemeal technology*".

The term " social technology " (and even more the term " social engineering "[1] which will be introduced in the next section) is likely to arouse suspicion, and to repel those who are reminded by it of the " social blueprints " of the collectivist planners, or perhaps even of the " technocrats ". I realise this danger, and to counter undesirable associations, I have added the word " piecemeal "; also, to express my conviction that " piecemeal tinkering " (as it is sometimes called), combined with critical analysis, is the main way to practical results in the social as well as in the natural sciences. The social sciences have developed very largely through the criticism of proposals for social improvements, or more precisely, by investigations designed to ascertain whether or not a certain political or economic action is likely to produce an expected or desired result. This approach[2], which might indeed be called the classical, is what I have in mind when speaking of the technological approach to social science, or of " piecemeal social technology ".

Technological problems in the field of social science may be of a " private " or of a " public " character. For example, investigations of the technique of business administration, or of the effects of improved working conditions upon output, belong to the first group, while investigations of the effects of prison reform, or of universal health insurance, or of the stabilisation of prices by means of tribunals, or of the introduction of new import duties, etc., upon, say, the equalisation of incomes, belong to the second group; and so do some of the most urgent practical questions of the day, such as the possibility of trade cycle control, or the question whether centralised " planning " in the sense of state management of production is compatible with an effective democratic control of the administration.

The emphasis upon the practical technological approach does not mean that the theoretical problems which will arise from the analysis of the practical problems should be excluded. On the contrary, it is one of my main points that the technological approach is likely to prove fruitful by giving rise to significant problems of a purely theoretical kind. But besides helping us in the fundamental task of selecting problems, it imposes a discipline on our speculative inclinations (which, especially in the field of sociology proper, are liable to lead us into the regions of metaphysics); for it forces us to submit our theories to definite standards, such as standards of clarity, and of the capability of being submitted to practical tests. The technological tendency may be expressed, perhaps, if we say that sociology in particular—and perhaps the social sciences in general—should look, not so much for " its Newton or its Darwin "[3] as for its Galileo, or its Pasteur.

This and previous references to an analogy between the methods of the social and the natural sciences are likely to arouse as much opposition as our choice of terms like " social technology " and " social engineering " (this in spite of the tendency expressed by the word " piecemeal "). I may say, therefore, that I fully appreciate the importance of the fight against dogmatic methodological naturalism or " scientism " (to use Professor Hayek's term). But I do not see why we should not utilise this analogy as far as it is fruitful, undeterred by the fact that it has been badly misused and misrepresented in certain quarters. Besides, we can hardly offer a stronger argument against these dogmatic naturalists than to show that some of the methods they attack are fundamentally the same as the methods used in the natural sciences.

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[1] For a defence of this term, see note 1 to section 21.

[2] Cp. F. A. von Hayek, ECONOMICA, Vol. XIII (1933), p. 123. ". . . economics developed mainly as the outcome of the investigation and refutation of successive Utopian proposals . . ."

[3] Cp. M. Ginsberg, in *Human Affairs* (ed. by R. B. Cattell and others), p. 180. It must be admitted, however, that mathematical economics shows that one social science at least has gone through its Newtonian revolution.

Another *prima facie* objection against what we call the technological approach is that it implies the adoption of an " activist " attitude towards the social order (cp. section 1), and that it is therefore liable to prejudice us against the anti-interventionist or " passivist " view that our dissatisfaction with existing social or economical conditions is due to our inability to understand its working, and that active intervention can only make matters worse. Now I must admit that I am certainly out of sympathy with this " passivist " view, and that I even believe that a policy of *universal* anti-interventionism is untenable, since its supporters are bound to recommend such political intervention as aims at preventing intervention. In spite of this, the technological approach as such is neutral in this matter (as indeed it ought to be), and by no means incompatible with anti-interventionism. On the contrary, I think that anti-interventionism involves a technological approach. For to assert that interventionism makes matters worse is to say that certain political actions would not have certain effects, viz., not the desired ones; and it is one of the most characteristic tasks of any technology to *point out what cannot be achieved.*

It is worth while to consider this point more closely. As I have shown elsewhere,[4] every natural law can be expressed by asserting that *such and such cannot happen*; that is to say, by a sentence of the form of the proverb: " You can't carry water in a sieve." For example, the law of conservation of energy can be expressed by: " You cannot build a perpetual motion machine "; and that of entropy by: " You cannot build a machine which is a hundred per cent. efficient." This way of formulating natural laws is one which makes their technological significance obvious and may therefore be called the " *technological form* " of a natural law. If we now consider anti-interventionism in this light, then we see at once that what it asserts is a set of sentences of the form: " You cannot achieve such and such results ", or perhaps, " You cannot achieve such and such ends without such and such concomitant effects." But this shows that anti-interventionism can be called a typically technological doctrine.

It is not, of course, the only one in the realm of social science. On the contrary, the significance of our analysis lies in the fact that it draws our attention to a really fundamental similarity between the natural and the social sciences. I have in mind the existence of sociological laws or hypotheses which are analogous to the laws of hypotheses of the natural sciences. Since the existence of such sociological laws or hypotheses (other than so-called " historical laws ") has often been doubted,[5] a number of examples may be given here: " You cannot introduce agricultural tariffs and at the same time reduce the cost of living."—" You cannot, in an industrial society, organise consumers' pressure groups as effectively as you can organise certain producers' pressure groups."—" You cannot have a centrally planned society with a price system that fulfils the main functions of competitive prices." Another group of examples may be taken from the realm of power politics: " You cannot introduce a political reform without causing some repercussions which are undesirable from the point of view of the ends aimed at." (Therefore, look out for them.)—" You cannot introduce a political reform without strengthening the opposing forces, to a degree roughly increasing with the significance of the reform." (This may be said to be the technological corollary of " There are always interests connected with the *status quo*.")—" You cannot make a revolution without causing a reaction." To these examples we may add two more, which may be called " Plato's law of revolutions " (from the eighth book of the *Republic*) and " Lord Acton's law of corruption ", respectively: " You cannot make a successful revolution if the ruling class is not weakened by internal dissension or defeat in war."—" You cannot give a man power over other men without tempting him to misuse it—a temptation which roughly increases with the amount of power wielded, and which very few are capable of resisting."[6]—Nothing is here assumed about the strength of the available evidence in favour of these hypotheses, in whose formulations there is certainly very much room for improvement. They are merely examples of the kind of statements which a piecemeal technology may attempt to establish.

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[4] Cp. my *Logik der Forschung* (1935), pp. 32ff. (Negated existential propositions.) The theory may be contrasted with Mill, *Logic*, Book V., Ch. V., § 2.

[5] Cp., for example, M. R. Cohen, *Reason and Nature*, p. 356ff. The examples in the text appear to refute this particular anti-naturalistic view.

(21) *Piecemeal versus Utopian Engineering.* Notwithstanding the objectionable associations which attach to the term " engineering ",[1] I shall use the term " piecemeal social engineering " to describe the practical application of the results of piecemeal technology. The term is useful since there is a need for a term covering such social activity, private as well as public, as utilises consciously all available technological knowledge[2] in order to realise some aim or end. Piecemeal social engineering resembles physical engineering in regarding such ends as beyond the province of technology. (All that technology may say about ends is, at most, whether or not they are compatible, or realisable.) In this it differs from historicism, which regards the ends of human activities as dependent on historical forces and so within its province.

Just as the main task of the physical engineer is to design machines and to remodel and service them, so that of the piecemeal social engineer is to design social institutions, and to reconstruct and run those already in existence. The term " social institution " is used here in a very wide sense, covering bodies of a private as well as of a public character. Thus we shall call a business, whether it is a small shop or an insurance company, an institution, and likewise a school, or an " educational system ", or a police force, or a Church, or a law court. The piecemeal technologist or engineer recognises that only a minority of social institutions are consciously designed while the vast majority have just " grown ", as the undesigned results of human actions.[3]

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[6] A similar formulation of this " law of corruption " is discussed by C. J. Friedrich in his very interesting and partly technological *Constitutional Government and Politics* (1937). He says of this law that " all the natural sciences cannot boast of a single ' hypothesis ' of equal importance to mankind " (p. 7). I do not doubt its importance; but I think that we may find countless laws of equal importance in the natural sciences, if we only look for them among the more platitudinous rather than among the more abstract laws. (Consider such laws as that men cannot live without food, or that vertebrates are bisexual.) Professor Friedrich insists upon the anti-naturalist thesis that " social sciences cannot benefit from applying the methods of the natural sciences to them " (*op. cit.*, p. 4). But he attempts, on the other hand, to base his theory of politics on a number of hypotheses of whose character the following passages (*op. cit.*, pp. 14ff.) may give an idea: " Consent and constraint are each a living force, generating power." Together, they determine " the intensity of a political situation ". And since " this intensity is determined by the absolute amount of either consent or constraint or both, it is perhaps most readily presented by the diagonal of the parallelogram of these two forces; consent and constraint. In that case its numerical value would equal the square root of the sum of the squares of the numerical values of consent and constraint ". This attempt at applying the Pythagorean theorem to a " parallelogram " (we are not told why it should be rectangular) of " forces " which are too vague to be measurable, seems to me an example not of anti-naturalism but of just that kind of naturalism or " scientism " from which, admittedly, the social sciences cannot benefit. It may be noted that these " hypotheses " can hardly be expressed in technological form, while for example the " law of corruption ", whose importance is very justly emphasised by Friedrich, can be so expressed.

[1] To the use of the term " social engineering " (in the " piecemeal " sense) it has been objected by Professor Hayek that the typical engineering job involves the centralisation of all relevant knowledge in a single head, while it is the specific character of all truly social problems that knowledge is to be utilised which cannot be so centralised. (Cp. Hayek, *Collectivist Economic Planning.*) I admit that this fact is of fundamental importance. It can be formulated by the technological hypothesis: " You cannot concentrate all relevant knowledge needed for such tasks as the satisfaction of personal needs, or the utilisation of specialised skill and ability." (A similar hypothesis may be proposed regarding the impossibility of centralising initiative in connection with similar tasks.) The use of the term " social engineering " may now be defended by pointing out that the engineer must use the technological knowledge embodied in these hypotheses which inform him of the limitations of his own initiative as well as of his own knowledge.

[2] Including, if available, the knowledge of the limitations of knowledge, as explained in the previous note.

[3] The two views that social institutions are either " designed " or " grown ", correspond to those of the Social Contract theorists and of their critics, for example, Hume. But Hume does not give up the " functional " or " instrumentalist " view of social institutions, for he says that men could not do without them. This position might be elaborated into a Darwinian explanation of the instrumental character of undesigned institutions (such as language); if they do not serve certain interests, they have no chance of surviving. According to this view, undesigned social institutions may develop as *unintended consequences of rational actions*; just as a road may be formed without any special intention by people who find it convenient to use a track already existing. It need hardly be stressed, however, that the technological approach is quite independent of all questions of " origin ".

But much as he may be impressed by this important fact, as a technologist or engineer he will look upon them from a " functional "[4] or " instrumental " point of view; that is to say, as means to certain ends, or as convertible to the service of certain ends; as machines rather than as organisms. This does not mean, of course, that he will overlook the fundamental differences between institutions and physical instruments. On the contrary, he will demand that the technologist should study the differences as well as the similarities, expressing his results in form of hypotheses. And indeed, it is not difficult to formulate hypotheses about institutions in technological form as is shown by the following very general specimen:[5] " You cannot construct foolproof institutions, that is to say, institutions whose functioning does not very largely depend upon the persons involved; at best, they can reduce the uncertainty of the personal element, by assisting those who work for the aims for which the institutions are designed, and on whose personal initiative and knowledge success largely depends. (Institutions are like fortresses. They are effective only if they are properly manned.) "

The characteristic approach of the piecemeal engineer is this. Even though he may perhaps cherish some ideals concerning society " as a whole ", regarding its general welfare, etc., he does not believe in the method of redesigning it as a whole. Whatever his ends, he tries to achieve them by small adjustments which can be continually re-adjusted and improved upon. His ends may be of varying character, for example, the accumulation of wealth or of power by certain individuals, or by certain groups; or the distribution of wealth and power; or the protection of certain " rights " of individuals or groups, etc. Public or political social engineering, accordingly, may have the most diverse tendencies, totalitarian as well as liberal. (Examples of far-reaching liberal programmes for piecemeal reform are given by W. Lippmann, under the title " The Agenda of Liberalism ".[6]) The piecemeal engineer (like Socrates) knows how little he knows. He knows that we can learn only from our mistakes. Accordingly, he will make his way step by step, carefully comparing the results expected with the results achieved, and always on the look out for the unavoidable unwanted consequences of any reform; and he will avoid undertaking reforms of a complexity and scope which make it impossible for him to disentangle causes and effects, and to know what he is really doing.

Such " piecemeal tinkering " does not agree with the political temperament of many " activists ". Their programme, which also has been described as a programme of " social engineering ", may be called " holistic " or " Utopian engineering ".

Holistic or Utopian social engineering, as opposed to piecemeal social engineering, is never of a " private " but always of a " public " character. It aims at re-modelling the " whole of society " in accordance with a definite plan or blueprint; it aims at " seizing the key positions "[7] and at extending " the power of the State . . . until the State becomes nearly identical with society ";[8] and it aims, furthermore, at controlling from these " key positions ", the historical forces that mould the future of the developing society, either by arresting this development, or else by foreseeing its course and adjusting society to it.

It may be questioned, perhaps, whether the piecemeal and the holistic approaches here described are fundamentally different, considering that no limitations of the scope of a piecemeal approach have been given. As this approach is understood here, constitutional reforms, for example, fall well within its scope; also, it is not intended to exclude the possibility that a series of piecemeal reforms may be inspired by one general tendency, for example, towards a greater equalisation of incomes. In this way, piecemeal methods may lead to changes in what is usually called the "class structure of society". Is there any difference, it may be asked, between such more ambitious kinds of piecemeal engineering and the holistic or Utopian approach? And this question may become even more pertinent if we consider that, when trying to assess the likely consequences of some proposed reform, the piecemeal technologist must do his best to trace the repercussions of any measure on the "whole" of society.

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[4] For the " functional " approach, see B. Malinowski, for example, " Anthopology as the Basis of Social Science," in *Human Affairs* (ed. Cattell) esp. pp. 206ff., and 239ff.

[5] This example which asserts that the efficiency of institutional " machines " is limited, and that the functioning of institutions depends on their being supplied with proper personnel, may be perhaps compared with the principles of thermodynamics, such as the law of conservation of energy (in the form in which it excludes the existence of a perpetual motion machine). As such, it may be contrasted with some more " scientistic " attempts to work out an analogy between the physical concept of energy, and some sociological concepts such as power; cp., for example, Russell's *Power* (1938), p. 10f., where such a scientistic attempt is being made. His main point, however, that the various " forms of power ", such as wealth, propagandist power, naked power, may be sometimes " converted " into one another, can hardly be expressed in technological form.

[6] W. Lippmann, *The Good Society* (1937), Ch. XI, pp. 203ff. See also W. H. Hutt, *Plan for Reconstruction* (1943).

[7] The expression is often used by K. Mannheim in his *Man and Society in an Age of Reconstruction*; cp., his Index, and, for example, pp. 269, 295, 320, 381. This book is the most elaborate exposition of a holistic and historicist programme known to me and therefore singled out here for criticism.

[8] Cp. Mannheim, *ibid.*, 337. The passage is more fully quoted in section 23, where it is also criticised. (See note 6 to section 23.)

In answering this question, I shall not attempt to draw a precise line of demarcation between the two methods, but rather refer to the fact that it is the holists who reject the piecemeal approach as being too modest. Their rejection, however, is not wholly in keeping with their practice; for in practice, they always fall back to a somewhat haphazard and clumsy, although ambitious and ruthless application of what is essentially a piecemeal method without its cautious and self-critical character. In other words, I do not assert that there are two radically different *methods* in practical use, but rather that there are two radically different *doctrines* concerning the possible methods of rational social reform; and I hold that the one of these doctrines is true, while the other is false and liable to lead to mistakes which are avoidable as well as grave.

A considerable difference between the Utopian or holistic and the piecemeal approach is therefore to be seen in the fact that while the piecemeal engineer approaches the problem with an open mind as to the scope of the reform, the holist has decided beforehand the possibility and necessity of a complete reconstruction. This has far-reaching consequences. It prejudices the Utopian against accepting certain sociological hypotheses that describe the limitations of institutional control; for example, the one mentioned above in this section, expressing the uncertainty due to the personal element. By *a priori* rejecting such hypotheses, the Utopian approach violates the principles of scientific method. On the other hand, problems connected with the uncertainty of the personal element force the Utopianist, whether he likes it or not, to try to control the personal element by institutional means, and to extend his programme so as to embrace not only the transformation of society according to plan, but also the transformation of man.[9] "The political problem, therefore, is to *organise human impulses* in such a way that they will direct their energy to the right strategic points, and steer the total process of development in the desired direction." It seems to escape the Utopianist that this programme implies an admission of failure, even before he launches it. For it substitutes for the demand that we build a new society fit for men and women to live in, the demand that we "mould" these men and women to fit into the new society. This, clearly, removes any possibility of testing the success or failure of the new structure. For those who do not like living in it only admit thereby that they are not yet fit to live in it; that their "human impulses" need further "organising". But without the possibility of tests, any claim that a "scientific" method is being employed evaporates. The holistic approach is incompatible with a truly scientific attitude.

Utopian engineering is not one of the main topics of this paper, but there are two reasons why it will be considered, in the next three sections, along with historicism. First, because under the name of collectivist (or centralised) planning, it is a very fashionable doctrine from which "piecemeal technology" and "piecemeal engineering" must be carefully distinguished. Secondly, because Utopianism, as well as resembling historicism in its hostility to the piecemeal approach, frequently occurs as part of a historicist ideology.

(22) *The Unholy Alliance of Historicism and Utopianism.* That there is an opposition between the two methodological approaches which I have called "piecemeal technology" and "historicism" has been clearly recognised by Mill. "There are two kinds of sociological inquiry", he wrote.[1] "In the first kind, the question proposed is, . . . for example, what would be the effect of . . . introducing universal suffrage, in the present condition of society . . . But there is also a second inquiry . . . In this . . . the question is, not what will be the effect of a given cause

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[9] "The Problem of Transforming Man" is the heading of a chapter of Mannheim's *Man and Society*. The following quotation is from this chapter, p. 199f.

[1] Cp. Mill, *Logic*, Book VI, Ch. X, § 1.

in a certain state of society, but what are the causes which produce . . . States of Society generally." Considering that Mill's "States of Society" correspond precisely to what we have called "historical periods", it is clear that his distinction between "two kinds of sociological inquiry" corresponds to our distinction between the approach of piecemeal technology and that of historicism; and this becomes even more obvious if we follow more closely Mill's description of the "second kind of sociological inquiry" which (under the influence of Comte) he declares to be superior to the first and which he describes as making use of what he calls "the historical method".

As has been shown before (in sections 1, 17 and 18), historicism is not opposed to "activism". A historicist sociology can even be interpreted as a kind of technology which may help (as Marx puts it) to "shorten and lessen the birth pangs" of a new historical period. And indeed, in Mill's description of the historical method we can find this idea formulated in a manner which is strikingly similar to that of Marx[2]: "The method now characterised is that by which . . . the laws . . . of social progress must be sought. By its aid we may hereafter succeed not only in looking far forward into the future history of the human race, but in determining what artificial means may be used . . . to accelerate the natural progress in so far as it is beneficial . . .[3] Such practical instructions, founded on the highest branch of speculative sociology, will form the noblest and most beneficial portion of the Political Art."

As indicated by this passage, it is not so much the fact that it is a technology as the fact that it is a *piecemeal* technology that marks the difference between our and the historicist approach. In so far as historicism is technological, its approach is not piecemeal, but "holistic" (cp. also section 7).

This holistic approach is very clearly indicated by Mill in his explanation of what he means by a "State of Society" (or historical period): "What is called a state of society", he writes,[4] ". . . is the simultaneous state of all the greater social facts or phenomena." Examples of these facts, *inter alia*, are: "The state of industry, of wealth and its distribution"; society's "division into classes, and the relations of those classes to one another; the common beliefs which they entertain . . .; their form in government, and the more important of their laws and customs." Summing up, Mill characterises states of society as follows: "States of society are like . . . different ages in the physical frame; they are conditions not in one or a few organs or functions, but of *the whole organism*."

It is this holism which distinguishes historicism most radically from any piecemeal technology, and which makes it possible for it to ally itself to a kind of holistic or Utopian social engineering.

This is certainly a somewhat strange alliance; for we have seen (in section 15) that there is a very definite clash between the approach of the historicist and that of the social engineer or the technologist, provided we understand by social engineering the construction of social institutions according to plan. From the point of view of historicism, the historicist approach is just as radically opposed to any kind of social engineering as the approach of a meteorologist to that of the magician who makes rain; accordingly, social engineering (even the piecemeal approach) is denounced,[5] by historicists, as Utopianism. In spite of that, we find historicism very frequently allied with just those ideas which are typical of holistic or Utopian social engineering, such as the idea of "blueprints for a new order", or of "centralised planning".

Two characteristic representatives of this alliance are Plato and Marx. Plato pessimistically believed that all change is decay; this was his law of historical development. Accordingly, his Utopian blueprint aims at arresting all change[6]; it is what nowadays, would be called "static". Marx, on the other hand, was an optimist, and possibly (like Spencer) an adherent of a historicist moral theory. Accordingly, his Utopian blueprint was one of a developing or "dynamic" rather than of an arrested society. He predicted and tried actively to further a development which culminates in an ideal Utopia that knows no political or economic coercion; the state has withered away, everybody co-operates freely in accordance with his abilities, and all his needs are satisfied.

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[2] *Logic*, Book VI, Ch. X, § 8. Marx's parallel passage (quoted above, in section 17 of this paper) is from his preface to the first edition of *Capital*.

[3] This remark shows that Mill's Utilitarianism prevented him from defining "beneficial" as synonymous with "progressive"; that is to say, in spite of his progressivism, he did not uphold a historicist moral theory (cp. section 19) such as was developed by Spencer and Engels (and nowadays by Waddington; cp. his *Science and Ethics*).

[4] Mill, *ibid.*, § 2 (italics mine).

[5] Cp. sections 15 to 17; see especially Engels' *Socialism, Utopian and Scientific*.

[6] I have discussed this at length in a forthcoming book, *The Open Society and Its Enemies*, that treats selected chapters of the history of historicism.

The strongest element in the alliance between historicism and Utopianism is, undoubtedly, the holistic approach which is common to both. Historicism is interested in the development, not of aspects of social life, but of " society as a whole "; and Utopian engineering is similarly holistic. Both overlook an important fact which will be established in the next section—the fact that " wholes " in this sense can never be the object of scientific inquiry. Both are dissatisfied with " piecemeal tinkering " and " muddling through ": they wish to adopt more radical methods. And both the historicist and the Utopianist seem to be impressed, and sometimes even deeply perturbed, by the experience of a changing social environment (an experience which often is frightening, and sometimes described as that of a " social breakdown "). Accordingly, they both attempt to rationalise this change, the one by prophesying the course of social development, and the other by clamouring that it should be either strictly and completely controlled, or even entirely arrested. The control must be complete, for in any compartment of social life which is not so controlled, there may lurk the dangerous forces which make for unforeseen changes.

Another link between the historicist and the Utopianist is that both believe that their aims or ends are not a matter of choice, or of a moral decision, but that they may be scientifically discovered by them within their fields of inquiry. (In this, they are opposed to the piecemeal technologist and engineer, just as much as to the physical engineer.) Both the historicist and the Utopianist believe that they can find out what the true aims or ends of " society " are, for example, by determining its historical tendencies, or by " diagnosing " the needs of their time. They are thus liable to adopt some kind of historicist moral theory (cp. section 18). It is no accident that most authors who advocate Utopian " planning " tell us that planning is simply inevitable, owing to the direction in which history is proceeding; and that we must plan, whether we like it or not.[7]

In the same historicist vein, these authors denounce their opponents for being mentally backward, and find that it is their main task " to break the old habits of thought, and to find the new keys to the understanding of the changing world "[8], asserting that trends of social change " cannot be successfully influenced or even deflected " as long as we do not give up the piecemeal approach, or " the spirit of 'muddling through' ". But it may be doubted whether the new " thought on the level of planning "[9] is as novel as all that, for holism seems to have been characteristic of fairly ancient thought, from Plato onward. And I rather think that a good case can be made for the view that the holistic way of thinking (whether about " society " or about " nature "), so far from representing a high level or late stage of the development of thought, is characteristic of a pre-scientific stage.

(23) *Criticism of Holism.* Having sketched the tendencies underlying my criticism, as well as the opposition between the piecemeal approach on the one hand and historicism and Utopianism on the other, I now proceed to my main task, the examination of the historicist claims. I begin with a brief criticism of holism (cp. section 7) since this has turned out, in the last sections, to be one of the most crucial positions of the theory to be attacked.

There is a fundamental ambiguity in the use of the word " whole " in recent holistic literature. It is used to denote (a) the totality of all the properties or aspects of a thing, and especially of all the relations holding between its constituent parts, and (*b*) certain selected properties or aspects of the thing in question, namely those that make it appear as an organised structure rather than as a " mere heap ". Wholes in sense (*b*) have been made the objects of scientific study, especially by the so-called "*Gestalt*" school of psychology; and there is indeed no reason why we should not study such aspects as the regularities of structure (for example, the symmetry) which can be found in certain things such as organisms, or electrical fields, or machines. Of things that possess such structures it may be said, as *Gestalt* theory puts it, that they are "more than the mere sum of their parts".

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[7] Cp., for example, K. Mannheim's *Man and Society*, p. 6 (and many other places) where we are told that " there is no longer any choice 'between planning and not planning', but only a choice 'between good planning and bad' "; or F. Zweig, *The Planning of Free Societies* (1942), p. 30, who answers the question whether a planned or an unplanned society is preferable by saying that this question does not arise, since it is solved for us by the direction of the historical development.

[8] K. Mannheim, *op. cit.*, p. 33; the next quotations are from *ibid.*, p. 7.

[9] K. Mannheim, not unlike Comte, distinguishes three " levels " in the development of thought: (1) trial and error or chance discovery, (2) invention, (3) planning (*ibid.*, p. 150f.). I am so far from agreeing with his doctrine that the trial and error method (1) appears to me to approach more closely the method of science than any of the other " levels ".—An additional reason for considering the holistic approach to social science as pre-scientific is that it contains an element of perfectionism. Once we realise that we cannot make heaven on earth, but can only improve matters a little, we also realise that we can only improve them little by little.

Any of the examples of *Gestalt* theory may be used to show that wholes in sense (b) are very different from wholes in sense (a). If, with the *Gestalt* theorists, we consider that a melody is more than a mere collection or sequence of single musical sounds, then it is *one of the aspects* of this sequence of sounds which we select for consideration. It is an aspect which may be clearly distinguished from other aspects, such as the absolute pitch of the first of these sounds, or their average absolute strength. And there are other *Gestalt* aspects which are even more abstract than the melody aspects, for example the rhythm of the melody; for by considering rhythm we neglect even relative pitch, which is important for melody. By thus being selective, the study of a *Gestalt*, and with it, of any whole in sense (b), is sharply distinguished from the study of a totality, i.e., of a whole in sense (a).

The fact that wholes in sense (b) can be studied scientifically must therefore not be appealed to in order to justify the entirely different claim that wholes in sense (a) can be so studied. The latter claim is undoubtedly false. By making anything the object of study, we must select certain aspects of it. It is not possible for us to observe, or to describe, a whole piece of the world, or a whole piece of nature; in fact, not even the smallest whole piece may be so described, since all description is necessarily selective.[1] It may even be said that wholes in sense (a) can never be the object of any activity, scientific or otherwise. If we take an organism and transport it to another place, then we deal with it as a physical body, neglecting many of its other aspects. If we kill it, then we have destroyed certain of its properties, but never all of them. In fact, we cannot possibly destroy the totality of its properties and of all the interrelations of its parts, not even if we smash or burn it.

But the fact that wholes in the sense of totalities cannot be made the object of scientific study, or of any other activity such as control or reconstruction, seems to have escaped the holists, even those of them who admit[2] that, as a rule, science is selective. They do not doubt the possibility of a scientific grasp of social wholes (in the sense of totalities) because they rely on the precedent of *Gestalt* psychology; for they believe[3] that the difference between the *Gestalt* approach and a treatment of social wholes in sense (a), embracing the "structure of all social and historical events of an epoch", lies merely in the fact that a *Gestalt* may be grasped by direct intuitive perception, while social wholes are "too intricate to be understood at a glance", so that they "can only be gradually grasped after long thought, in which all elements are noted, compared, and combined". They do not see, in short, that *Gestalt* perception simply has nothing to do with wholes in sense (a), that all knowledge, whether intuitive or discursive, must be of abstract aspects, and that we can never grasp the "concrete" structure "of social reality itself".[4] Having overlooked this point, they insist[5] that the specialist's study of "petty details" must be complemented by an "integrating" or "synthetic" method which aims at reconstructing "the whole process"; and they assert that "sociology will continue to ignore the essential question as long as specialists refuse to see their problems as a whole ". But this holistic method by necessity remains a mere programme. Not one example of a scientific description of a whole, concrete social situation is ever quoted. And it cannot be quoted, since in every such case it would always be easy to point out aspects which have been neglected, in spite of the fact that they may be important in some context or other.

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[1] H. Gomperz, *Weltanschauungslehre* II/I (1908), p. 63, points out that a piece of the world, such as a sparrow anxiously fluttering about, may be described by the following widely different propositions, each corresponding to a different aspect of it: "This bird flies!"—"There flutters a sparrow!"—"Look, here is an animal!"—"Something moves here."—"Energy is being transformed here."—"This is not a perpetual motion."—"The poor thing is frightened!" It is clear that it can never be the task of science to attempt the completion of such a list, which necessarily is infinite.—F. A. von Hayek, in *Ethics*, Vol. LIV (1943), note 5, sketches a criticism of holism which is very similar to the one propounded here in the text.

[2] K. Mannheim describes (*op. cit.*, p. 167) selective or abstract science as "a stage through which all sciences which strive for precision must pass".

[3] With the following quotations, cp. Mannheim, *op. cit.*, p. 184; cp. also p. 170, note, and p. 230.

[4] *Ibid.*, p. 230. The doctrine that we may obtain a kind of concrete knowledge of "reality itself" is well known as a part of what can be technically described as ***mysticism***; and so is the clamouring for "wholes".

[5] Cp. *op. cit.*, for example, pp. 26 and 32. My criticism is not directed against a plea for co-operation between the various branches of science; especially when we are faced with a definite piecemeal problem which might be furthered by such co-operation, nobody would dream of opposing it. But this is a very different matter from the plan to grasp concrete wholes by a method of systematic synthesis, or something of that sort.

But the holists do not plan only the study of society by a non-existent method, they plan also the control and the reconstruction of society as a whole. They prophecy that[6] " the power of the State is bound to increase until the State becomes nearly identical with society ". The intuition expressed by this passage is clear enough. It is the totalitarian intuition.[7] Yet what, apart from conveying this intuition, does the prophecy mean? The term " society " embraces, of course, all social relations, including all personal ones; those of a mother to her child as much as those of a child welfare officer to either of the two. It is for many reasons quite impossible to control all, or " nearly " all, these relationships; if only because with every new control of social relations we create a host of new social relations to be controlled. That is to say, the impossibility is a logical one.[8] (The attempt leads to an infinite regression; the position is similar in the case of an attempt to *study* the whole of society.) Yet there can be no doubt that the Utopianists plan to attempt precisely this impossibility, for they assure[9] us that " it will be possible ", among other things, even " to mould personal intercourse in a more realistic way ". (Nobody doubts, of course, that wholes in sense (b) can be moulded or controlled or even created, as opposed to wholes in sense (a); we can create a melody, for example. But this has nothing to do with Utopian dreams of total control.)

So much about Utopianism. As far as historicism is concerned (and such arguments as are propounded in section 7), the position is similarly hopeless. Historicist holists often assert, by implication, that the historical method is adequate for the treatment of wholes in the sense of totalities. But this assertion[10] rests on a misunderstanding. It arises from combining the correct belief that history, as opposed to the theoretical sciences, is interested in concrete individual events and in individual personalities rather than in abstract general laws, with the mistaken belief that the " concrete " individuals in which history is interested can be identified with " concrete " wholes in sense (a). But they cannot; for history, like any other description, can only deal with selected aspects of the object in which it is interested. It is mistaken to believe that there can be a history in the holistic sense, a history of " States of Society " which represent " the whole of the social organism " or " all the social and historical events of an epoch ". This idea derives from an intuitive view of a *history of mankind* as a vast and comprehensive stream of development. But such a history cannot be written. Every written history is a history of a certain narrow aspect of this " total " development, and is anyhow a very incomplete history even of the particular incomplete aspect chosen.

The holistic tendencies of Utopianism and of historicism are unified in the following characteristic statement[11]: "We have never had to set up and direct the entire system of nature as completely as we are forced to do to-day with our society, and *therefore* we have never had to penetrate into the history and structure of the individual worlds of nature. Mankind is tending . . . to regulate the whole of its social life although it has never attempted to undertake the creation of a second nature . . ." This statement illustrates the mistaken belief that if we wish, holistically, to treat " the entire system of nature completely ", we should be helped in doing so by adopting a historical method. But natural sciences such as geology which have adopted this method are far from grasping the " entire system " of their subject matter. Also illustrated by this statement is the incorrect view that it is possible to " set up " or " direct " or " regulate " or " create " wholes in sense (a). That " we have never had to set up and direct the entire system of nature " is certainly true, simply because we cannot even set up and direct one single piece of physical apparatus in its " entirety ". Such things cannot be done. They are Utopian dreams, or perhaps misunderstandings. And to tell us that we are *forced* to-day to do a thing which is logically impossible, viz., to set up and direct the entire system of society, and to regulate the whole of social life, is merely a typical attempt to threaten us with " historical forces " and " impending developments " which make Utopian planning inevitable.

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[6] Cp. *op. cit.*, p. 337: and note 8 to section 21.

[7] The formula is nearly identical with one by C. Schmitt.

[8] Holists may hope that there is a way out of this difficulty by denying the validity of logic, which they think has been superseded by dialectic. This way I have tried to block in " What is Dialectic? ", *Mind*, Vol. 49 N.S., pp. 403ff.

[9] Cp. K. Mannheim *op. cit.*, p. 202. It may be mentioned that holism is at present very fashionable with educational theorists.

[10] The doctrine that history deals with " concrete individual wholes ", which may be persons or events or epochs, was propagated especially by Troeltsch. Its truth is constantly assumed by Mannheim.

[11] K. Mannheim, *op. cit.*, p. 175f.

Incidentally, the statement quoted is interesting as an admission of the very significant fact that there exists no physical analogy of holistic engineering or of the corresponding " science ". The pursuit of the analogy between natural and social science is therefore certainly helpful in clarifying the issue here.

Such is the logical status of holism, the rock on which we are encouraged to build a new world.

In conclusion, a critical remark may be made concerning the wholes in sense (b), whose scientific status has been admitted above. Without retracting anything said before, it may be pointed out that the triviality as well as the vagueness of the statement that the whole is more than the sum of its parts seems to be seldom realised. Even three apples on a plate are more than " a mere sum ", in so far as there must be certain relations between them (the biggest may or may not lie between the two others, etc.), which do not follow from the fact that they are three, and which can be studied scientifically. Also, the much advertised opposition between the " atomistic " and the " *Gestalt* " approach is entirely baseless, at least as far as atomic physics is concerned; for atomic physics[12] does not merely " sum up " its elementary particles, but studies particle systems from a point of view most definitely concerned with wholes in sense (b).

What most of the *Gestalt* theorists wish to assert is, apparently, the existence of two kinds of things, viz., " heaps ", in which we cannot discern any order, and " wholes ", in which an order or symmetry or a regularity or a system or a structural plan may be found. Thus a sentence such as " Organisms are wholes " reduces itself to the triviality that, in an organism, we can discern some order. Besides, a so-called " heap ", as a rule, has a *Gestalt* aspect too, just as well as the often cited example of the electrical field (consider the typical way in which pressure increases within a heap of stones). Thus the distinction is not only trivial, but exceedingly vague; and is not applicable to different kinds of things, but merely to different aspects of the same things.

(24) *The Holistic Theory of Social Experiments*. Holistic thinking is particularly deterimental in its influence upon the historicist theory of social experiments (expounded above in section 2). Although the piecemeal technologist will agree with the historicist view that large-scale or holistic social experiments, if at all possible, are extremely unsuitable for scientific purposes, he will emphatically deny the assumption, common to both historicism and Utopianism, that social experiments, in order to be realistic, must be of the character of Utopian attempts at re-modelling the whole of society.

It is convenient to begin our criticism with the discussion of a very obvious objection to the Utopian programme, namely, that we do not possess the experimental knowledge needed for such an undertaking. The blueprints of the physical engineer are based on an experimental technology; all the principles that underlie his activities are tried out by practical experiments. But the holistic blueprints of the social engineer are not backed by any comparable practical experience. Thus the alleged analogy between physical engineering and holistic social engineering breaks down; holistic planning is rightly described as " Utopian ", since the scientific basis of its plans is simply non-existent.

Faced with this criticism, the Utopian engineer is likely to grant the need for practical experience, and for an experimental technology. But he will claim that we shall never know anything about these matters if we recoil from making social experiments, or, what in his view amounts to the same, from holistic engineering. We must make a beginning, he will argue, using whatever knowledge we possess, be it great or small. If we have some knowledge of aircraft designing to-day, it is only because some pioneer who did not possess this knowledge dared to design an aircraft and to try it out. Thus the Utopianist may even contend that the holistic method which he advocated is nothing but the experimental method applied to society. For he holds, in common with the historicist, that small scale experiments, such as an experiment in socialism carried out in a factory or in a village or even in a district, would be quite inconclusive; such isolated " Robinson-Crusoe-experiments " cannot tell us anything about modern social life in the " Great Society ". They even deserve the attribute " Utopian " in the (Marxist) sense in which this term expresses the neglect of historical tendencies: in this case, of the tendency towards an increasing interdependence of social life.

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[12] Cp., for example, Pauli's exclusion principle.—To the social scientist, such ideas as competition or division of labour should make it abundantly clear that an " atomistic " or " individualistic " approach is in no way opposed to an appreciation of the fact that every individual interacts with all others. (In psychology, the situation is different, simply because atomism seems to be inapplicable there.)

We see that Utopianism and historicism agree in the view that a *social experiment, if at all practicable, would be of value only if carried out on a holistic scale.* It follows from this widely held prejudice that we are very rarely in the position deliberately to carry out " planned experiments ", and that, for an account of the results of the " chance experiments " so far carried out in the social field, we have to turn to *history.*[1]

Against this view, we shall offer two objections: (a) that it overlooks those piecemeal experiments which are fundamental for all social knowledge, pre-scientific as well as scientific; (b) that holistic experiments are unlikely to contribute much to our experimental knowledge, and that they can be called " experiments " only in the sense in which this term is synonymous with *an action whose outcome is uncertain*, but not in the sense in which this term is used to denote *a means of acquiring knowledge, by comparing the results obtained with the results expected.*

Concerning (a), it may be pointed out that the holistic view of social experiments leaves unexplained the fact that we possess a very great deal of experimental knowledge of social life. There is a difference between an experienced and an inexperienced business man, or organiser, or politician, or general. It is a difference in their social experience; and in experience gained not merely by contemplating what they have observed, but by efforts to achieve something practical. It must be admitted that the knowledge attained in this way is usually of a pre-scientific character, and that in this respect it may resemble knowledge gained by casual observation, rather than knowledge gained by carefully designed scientific experiments; but this is no reason for denying that the knowledge in question is based on experiment rather than on mere observation. A grocer who opens a new shop is conducting a social experiment; and even a man who joins a queue before a theatre gains experimental technological knowledge which he may utilise by having his seat reserved next time, which again, is a social experiment. And we should not forget that only practical experiments have taught buyers and sellers on the markets the lesson that prices are liable to be lowered by every new supply, and raised by every new demand.

Piecemeal experiments on a somewhat larger scale are, for example, the decision of a monopolist to change the price of his product; the introduction, by a private or public insurance company, of a new type of health or employment insurance; or the introduction of a new sales tax, or of an anti-cycle policy. All these experiments are carried out with practical rather than scientific aims in view; and yet, some experiments with prices, for example, have been carried out, by some large firms, with the direct intention of increasing their knowledge of the market (in order to increase profits at a later stage, of course) rather than with the aim of increasing their profits immediately.[2] The situation is very similar to that of physical engineering and to the pre-scientific methods by which our technological knowledge in matters such as the building of ships or the art of navigation was first acquired. There seems to be no reason why these methods should not be improved on, and ultimately replaced by a more scientifically minded technology; that is to say, by an approach in the same direction, and based on critical thought as well as on experiment.

According to this piecemeal view, there is no absolute barrier between the pre-scientific and the scientific experimental approaches, even though the more and more conscious application of scientific, that is to say, of critical methods, is of great importance. Both approaches may be described, fundamentally, as utilising the method of trial and error. We try, i.e., we are not merely registering our observations, but we are actively engaged in the solution of some more or less practical and definite problem. And we are making progress because, and only if, we are prepared to *learn from our mistakes*, that is to say, to recognise our errors and to utilise them critically instead of persevering in them dogmatically. Though this analysis may sound trivial, it describes, I believe, the method of all empirical sciences. This method assumes a more and more scientific character the more freely and consciously we are prepared to risk a trial, and the more critically we are looking out for the mistakes we always make. And this formula covers not only the method of experiment, but also the relationship between theory and experiment. All theories are trials; they are tentative hypotheses, tried out to see whether they work; and all experimental confirmation is nothing but the result of tests undertaken in a critical spirit, in an attempt to find out where our theories err.[3]

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[1] This was also Mill's view when he said of social experiments that " we palpably never have the power of trying any. We can only watch those which nature produces, . . . the successions of phenomena recorded in history . . . " (cp. *Logic*, Book VI., Ch. VII, § 2).

[2] Sidney and Beatrice Webb, *Methods of Social Study* (1932), pp. 221ff., give similar examples of social experiments. They do not distinguish, however, between the two kinds of experiments which are here called " piecemeal " and " holistic " experiments, although their criticism of the experimental method (cp. p. 226, " intermixture of effects ") is especially justified as a criticism of holistic experiments (which they seem to admire). Furthermore, their criticism is combined with the " variability argument " which I consider to be invalid; cp. section 25, below.

For the piecemeal technologist or engineer these views mean that, if he wishes to introduce scientific methods into the study of society and into politics, what is needed most is the adoption of a critical attitude, and the insight that not only trial but also error is necessary. And he must learn not only to expect mistakes, but consciously to search for them. We all have an unscientific weakness for being always in the right, and this weakness seems to be particularly common among professional and amateur politicians. But the only way leading to something like scientific method in politics is to proceed on the assumption that there can be no political move which has not its drawbacks, its undesirable consequences. To look out for these mistakes, to find them, to bring them into the open, to analyse them, and to learn from them, this is what a scientific politician as well as a political scientist must do. Scientific method in politics means that the great art of convincing ourselves that we have not made any mistakes, of ignoring them, of hiding them, and of blaming others for them, is replaced by the greater art of accepting the responsibility for them, of trying to learn from them, and of applying this knowledge so that we may avoid them in the future.

We now turn to point (b), the criticism of the view that we can learn from holistic experiments, or more precisely, from measures carried out on a scale that approaches the holistic dream (for holistic experiments in the radical sense that they re-model " the whole of society " are logically impossible, as shown in the foregoing section). Our main point is very simple: it is difficult enough to be critical of our own mistakes, but it must be nearly impossible for us to adopt such a critical attitude toward those of our actions which involve the lives of many men. To put it differently, it is very hard to learn from very big mistakes.

The reasons for this are twofold; they are technical as well as moral. Since so much is done at a time, it is impossible to say which particular measure is accountable for any of the results; or rather, if we do attribute a certain result to a certain measure, then we can do so only on the basis of some theoretical knowledge gained previously, and not from the holistic experiment in question. This experiment does not help us to attribute particular results to particular measures; all we can do is to attribute the " whole result " to it; and whatever this may mean, it is certainly difficult to assess. Even the greatest efforts to secure a well-informed, free, and critical statement of these results are unlikely to prove successful. But the chances that such efforts will be made are negligible; on the contrary, there is every likelihood that free discussion about the holistic plan and its consequences will not be tolerated. The reason is that every attempt at planning on a very large scale is an undertaking which must cause considerable inconvenience to many, and for a considerable span of time. Accordingly, there will always be a tendency to oppose the plan, and to complain about it. To many of these complaints, the Utopian engineer will have to turn a deaf ear, if he wishes to get anywhere at all; in fact, it will be part of his business to suppress unreasonable objections. But with it, he must invariably suppress reasonable criticism too. And the mere fact that the expression of dissatisfaction will have to be curtailed reduces even the most enthusiastic expression of satisfaction to insignificance. Thus it will be difficult to ascertain the facts, i.e., the repercussions of the plan on the individual citizen; and without these facts, a scientific criticism is impossible.

But the difficulty of combining holistic planning with scientific methods is still more fundamental than so far indicated. The holistic planner overlooks the fact that it is easy to centralise power but impossible to centralise all that knowledge which is distributed over many individual minds,[4] and whose centralisation would be necessary for wielding centralised power wisely. Unable to ascertain what is in the minds of so many individuals,[5] he must try to simplify his problems by eliminating individual differences; he must try to control and to stereotype interests and beliefs by education and propaganda. But this attempt to exercise power over minds must destroy the last possibility of finding out what people really think, for it is clearly incompatible with free thought, especially critical thought. The more you gain in power, the more you must lose in knowledge. (Political power and social knowledge are " complementary " in Bohr's sense of the term.[6])

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[3] A fuller analysis of the methods of modern physics on the lines here indicated may be found in my *Logik der Forschung* (1935); cp. also " What is Dialectic ? ", *Mind*, Vol. 49, pp. 403ff. Cp. also for example Tinbergen, *Statistical Testing of Business Cycle Theories*, Vol. II, p. 21: " The construction of a model . . . is . . . a matter of trial and error ", etc.

All these remarks are confined to the problem of scientific method. They tacitly grant the colossal assumption that we need not question the fundamental benevolence of the planning Utopian engineer, who is vested with an authority which at least approaches dictatorial powers. Tawney concludes a discussion of Luther and his time with the words: " Sceptical as to the existence of unicorns and salamanders, the age of Machiavelli and Henry VIII found food for its credulity in the worship of that rare monster, the God-fearing Prince." Replace here the words " unicorns and salamanders " by " ordinary citizens without ulterior motives "; replace the two names by those of two of their modern counterparts, and the phrase " the God-fearing Prince " by " the benevolent planning authority "; and you have a description of the credulity of our own time. This credulity shall not be challenged here; yet it may be remarked that, assuming the unlimited and unvarying benevolence of the powerful planners, our analysis shows that it may be impossible for them ever to find out whether the results of their measures tally with their good intentions.

I do not believe that any corresponding criticism of the piecemeal method can be offered. This method can be used, more particularly, in order to search for and fight against the greatest and most urgent evils of society, rather than to seek and to fight for some ultimate good (as holists are inclined to do). But a systematic fight against definite wrongs, against concrete forms of injustice or exploitation, and avoidable suffering such as poverty or unemployment, is a very different thing from the attempt to realise a distant ideal blueprint of society. Success or failure is more easily appraised, and there is no inherent reason why this method should lead to an accumulation of power, and to the suppression of criticism. Also, such a fight against concrete wrongs and concrete dangers is more likely to find the support of a great majority than a fight for the establishment of a Utopia, ideal as it may appear to the planners. This may perhaps throw some light on the fact that in democratic countries defending themselves against aggression, sufficient support may be forthcoming for far-reaching measures in the direction of holistic planning[7] without suppression of public criticism, while in countries preparing for an attack or waging an aggressive war, public criticism as a rule must be suppressed, so that public support may be mobilised by presenting the attack as defence.

We may now turn back to the Utopianist's claim that his method is the true experimental method applied to the field of society. This claim, I think, is dispelled by our criticism. This can be further illustrated by the analogy between physical and holistic engineering. It may be admitted that physical machines can be successfully planned by way of blueprints, and with them, even a whole plant for their production, etc. But all this is possible only because many piecemeal experiments have been carried out beforehand. Every machine is the result of a great many small improvements. Every model must be " developed " by the method of trial and error, by a countless number of small adjustments. The same holds for the planning of the production plant. The apparently holistic plan can succeed only because we have made all kinds of smaller mistakes already; otherwise there is every reason to expect that it would lead to big mistakes.

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[4] The observation that it is impossible to have the knowledge needed for planning " concentrated anywhere in a single head ", is due to Hayek; cp. *Collectivist Economic Planning*, p. 210. (See also note 1 to section 21, above.)

[5] One of the most crucial points in Spinoza's political theory is the impossibility to know, and to control, what other people think. He defines " tyranny " as the attempt to achieve the impossible, and to exercise power where it cannot be exercised. Spinoza, it must be remembered, was anything but a liberal; he did not believe in institutional taming of power, but thought that a prince might exercise his powers up to their actual limit. Yet what Spinoza calls " tyranny ", and declares to be in conflict with reason, is treated quite innocently by holistic planners as a " scientific " problem: the " problem of transforming men ".

[6] Niels Bohr calls two approaches " complementary " if they are (a) complementary in the usual sense and (b) if they are exclusive of each other in the sense that the more we make use of the one the less we can use the other. Although I refer in the text mainly to *social* knowledge, it may be claimed that the accumulation of political power is " complementary " to the progress of scientific knowledge in general. For the progress of science depends on free competition of thought, hence on freedom of thought, and hence, ultimately, on political freedom.

[7] R. H. Tawney, *Religion and The Rise of Capitalism*, Ch. II, end of section (ii).

Thus the analogy between physical and social engineering, if looked into more closely, turns against the holist and in favour of the piecemeal social engineer. The expression " social engineering ", which alludes to this analogy, has been usurped by the Utopianist without a shadow of right.

With this, we conclude our critical remarks on Utopianism, in order to concentrate our attack on its ally, historicism. To the historicist's contentions concerning social experiments, our argument provides, I think, a sufficient answer, except for his claim that social experiments are not useful because of the impossibility of repeating them under precisely similar conditions. We now turn to the discussion of this argument.

(25) *The Variability of Experimental Conditions.* The historicist contention that the experimental method is inapplicable to the social sciences since, in contrast to the situation prevalent in physics, we cannot reproduce at will precisely similar experimental conditions, leads us a little closer to the heart of the historicist position. Fundamentally, this contention rests upon a gross misunderstanding of the experimental methods of physics, even though there is perhaps something in it, and correspondingly a certain difference between physical and sociological methods. But this difference is certainly not of a fundamental character.

We may first consider the methods of physics. Every experimental physicist knows that very dissimilar things may happen under what appear to be precisely similar conditions. Two wires may look exactly alike, but if the one is exchanged for the other in an electrical apparatus, the resulting difference may be very great. Upon closer inspection, we may perhaps find that they were not as similar as they first appeared. But often it is very hard indeed to detect a difference in the conditions of two experiments that lead to different results. Long researches, experimental as well as theoretical, may be needed in order to find what kind of similarity is relevant, and what degree of similarity sufficient; researches which may have to be carried out before we are able to secure similar conditions for our experiments, and before we even know what " similar conditions " means in this case. And yet, *the method of experiment is applicable.*

Thus we can say that what are to be described as " similar conditions " depend on the kind of experiment, and can be discovered only by using experiments. It is impossible to decide *a priori* about any difference or similarity intuitively grasped, however striking, whether or not it will be relevant for the purpose of reproducing an experiment. Rather, we must allow the experimental method to take care of itself.[1]

In the light of such considerations, the historicist argument that social experiments are fatally hampered by the variability of social conditions, and especially by the changes which are due to historical developments, loses its force. The striking differences with which the historicist is so much preoccupied, that is to say, the differences between the conditions prevalent in various historical periods, need not create any difficulties peculiar to social science. It may be admitted that if we were suddenly transported into another historical period, we should probably find that many of our social expectations, formed on the basis of piecemeal experiments made in our society, led to disappointment. In other words, experiments may lead to unforeseen results. But it would be *experiments* which led us to discover the change in social conditions; experiments would teach us of certain social conditions that they vary with the period of history; just as experiments have taught the physicist that the temperature of boiling water may vary with geographical position. In other words, the doctrine of the difference between historical periods, far from making social experiments impossible, is merely an attempt to formulate our assumption that, if shifted into another period, we should continue to make our piecemeal experiments, yet with surprising or disappointing results. In fact, if we know anything about different attitudes in different historical periods, then it is from experiments, carried out in our imagination. Historians find difficulties in interpreting certain records, or they discover facts showing that some of their predecessors had misinterpreted some historical evidence. These difficulties of historical interpretation are our only evidence of the kind of historical change the historicist has in mind; yet they are nothing but discrepancies between the expected and the actual results of our mental experiments. It is these surprises and disappointments which, by the method of trial and error, have led to improvements in our ability to interpret strange social conditions, that is to say, to adjust ourselves to them. And what in the case of historical interpretation we achieve by mental experiment, that has been achieved by some anthropologists in practical field work. Those modern investigators who succeeded in adjusting themselves to conditions which were probably no less remote than those of the Stone Age, owe their success to piecemeal experiments.

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[1] Precisely analogous considerations hold for the much debated problem of the artificial *isolation* of experiments against disturbing influences. Clearly, we cannot isolate a piece of apparatus against *all* influences; for example, we cannot know *a priori* whether the influence of the position of the planets or of the moon upon a physical experiment is considerable or negligible. What kind of artificial isolation, if any, is needed, we can learn only from the result of experiments, or from theories which, in turn, are tested by experiments.

Some historicists doubt the possibility of such successful adjustments; and they even defend their doctrine of the futility of social experiments by the argument that, if shifted to remote historical periods, far too many of our social experiments would lead to disappointments. They assert that we should be unable to adjust our habits of thought, and especially our habits of analysing social events, to these bewildering conditions. Such fears seem to me part of the historicist hysteria which exaggerates the importance of social change; but I must admit that it would be difficult to dispel them on *a priori* grounds. After all, the ability to adjust oneself to a new environment varies from person to person, and there seems to be no reason why we should expect of a historicist (who holds such defeatist views) that he will be able to adapt his mind successfully to changes in the social environment. Also, matters will depend on the character of the new environment. The possibility that a social investigator may find himself being eaten before he succeeds in adjusting himself, by trial and error, to cannibal habits, cannot be excluded any more than that, in some " planned " society, his investigations may end in a concentration camp. Yet analogous remarks hold for the realm of physics. There are many places in which physical conditions prevail which offer to the physicist little chance of adjusting himself by trial and error.

Summing up, there does not seem any basis for the specious historicist assertion that the variability of historical conditions renders the experimental method inapplicable to the problems of society, and for the assertion that, on this point, the study of society is fundamentally different from the study of nature. It is a quite different matter if we admit that, in practice, it is often very difficult for the social scientist to choose and to vary his experimental conditions at will. The physicist is in a better position although he too is sometimes faced by similar difficulties. Thus the possibilities of carrying out experiments in varying gravitational fields, or under extreme temperature conditions, are rather limited. But we must not forget that many possibilities which are open to the physicist to-day were impracticable not long ago, not because of physical but because of social difficulties, i.e., because men were not prepared to risk the sums needed for research. It is a fact, however, that very many physical investigations can now be carried out under experimental conditions which leave little to be desired, while the social scientist is in a very different position. Many experiments which would be most desirable will remain dreams for a long time to come, in spite of the fact that they are not of a Utopian but of a piecemeal character. In practice, he must rely too often on experiments carried out mentally, and on an analysis of political measures carried out under conditions and in a manner which leaves much to be desired from the scientific point of view.

(26) *Are Generalisations Confined to Periods?* The fact that I have discussed the problem of social experiments before discussing at any length the problem of sociological laws, or theories, or hypotheses, or " generalisations ", should not be interpreted as an indication that I consider observations and experiments as being in some sense or other logically prior to theories. On the contrary, I believe that theories are prior to observations as well as to experiments, in the sense that these are significant only in relation to theoretical problems. Also, we must have a question before we can hope that observation or experiment may help us in any way to provide an answer. Or to put it in terms of the method of trial and error, the trial must come before the error; and as we have seen (in section 24), the theory or hypothesis, which is always tentative, is part of the trial, while observation and experiment help us to weed out theories by showing where they err. Therefore I do not believe in the

" method of generalisation ", that is to say, in the view that science begins with observations from which it derives its theories by some process of generalisation or induction. I believe, rather, that the function of observation and experiment is the more modest one of helping us to test our theories and to eliminate those which do not stand up to tests; even though it must be admitted that this process of weeding out not only checks theoretical speculation, but also stimulates it to try again—and often to err again, and to be refuted again, by new observations and experiments.

In this section, I shall criticise the historicist contention (cp. section 1) that in the social sciences, the validity of all or at least of the most important generalisations is confined to the concrete historical period in which the relevant observations were made. I shall criticise this contention without first discussing the question whether or not the so-called " method of generalisation " is defensible, in spite of my conviction that it is not; for I think that the historicist contention can be refuted without assuming that this method is invalid. The discussion of my views on this method, and on the relations between theory and experiment in general, can therefore be postponed. It will be taken up in section 28.

I begin my criticism of the historicist contention with the admission that most people living in a certain historical period will incline to the erroneous belief that the regularities which they observe are universal laws of social life, holding good for all societies. Indeed, we sometimes become aware of cherishing such beliefs only when, in a foreign country, we find our habits regarding food, our greeting taboos, etc., are by no means as universal as we naïvely assumed. It is a rather obvious inference that many of our other generalisations, whether consciously held or not, may be of the same kind, though they may remain unchallenged since we cannot travel into another historical period. (This inference[1] was drawn, for example, by Hesiod.) In other words, it must be admitted that there may be many regularities in our social life which are characteristic of our particular period only, and that we are inclined to overlook this limitation, so that, in a time of rapid social changes, we may learn to our sorrow that we have relied on laws that have lost their validity.[2]

If the historicist's contentions went no further than this, no charge could be made against him except that he is labouring a rather trivial point. But unfortunately, he asserts more. He insists that the situation creates difficulties which do not occur in the natural sciences; and more particularly that, in opposition to the natural sciences, in the social sciences we must never assume that we have discovered a truly universal law, since we can never know whether it always held good in the past (for some of our records are not sufficient), or whether it will always hold good in the future.

In opposition to such claims, I do not admit that the situation described is in any way peculiar to the social sciences, or that it creates any particular difficulties. On the contrary, it is obvious that a change in our physical environment may give rise to experiences which are quite analogous to those which arise from a change in social environment. Can there be a more obvious and proverbial regularity than the succession of day and night? And yet, it breaks down if we cross the polar circle. It is a little difficult to compare such matters, but I think that such a breakdown is quite as startling as any that might occur in the social realm. To take another example, the social environments of Crete in 1900 and of Crete three thousand years ago can hardly be claimed to differ more than the two physical environments of Crete and of Greenland; at least, I think that a sudden unprepared removal from one physical environment into the other would be more likely to produce fatal results than a corresponding change in the social environment.

It seems clear to me that the historicist overrates the significance of the somewhat spectacular differences between the various historical periods, and that he underrates the possibilities of scientific ingenuity. It is true that the laws discovered by Kepler are valid only for planetary systems, but their validity is not confined to the

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[1] This inference forms the basis of the so-called " sociology of knowledge ".

[2] K. Mannheim, *Man and Society*, p. 178, writes about " the layman who observes the social world intelligently ", that " in static periods he is unable, in any case, to distinguish between a general abstract social law and particular principles which obtain only in a certain epoch, since in periods of only slight variability, the divergences between these two types do not become clear to the observer." Mannheim calls these particular principles which obtain only in a certain epoch " *principia media* "; cp. note 4, below. For the situation " in an age where the social structure is changing through and through ", cp. Mannheim, *op. cit.*, p. 179f.

Solar system in which Kepler lived, and which he observed.[3] Newton did not have to withdraw into a part of the universe where he could observe moving bodies that were free from the influence of gravitational and other forces, in order to see the importance of the law of inertia. On the other hand, this law, even though no body in this system moves in accordance with it, does not lose its significance within the Solar system. Similarly, there seems no reason why we should be unable to frame sociological theories which are important for all social periods. The spectacular differences between these periods are no indication that such laws cannot be found, any more than the spectacular differences between Greenland and Crete can prove that there are no physical laws which hold for both regions. On the contrary, these differences seem to be, in some cases at least, of a comparatively superficial character (such as are differences in habits, in greeting taboos, etc.), and more or less the same may be said of those regularities which are said to be characteristic of a period or a society (and which are now[4] sometimes called *principia media*).

To this, the historicist may reply that the differences in the social environment are more fundamental than the differences in the physical environment; for if society changes, man changes too; but this implies a change in all regularities, since all social regularities depend on the nature of man, the atom of society. Our answer is that physical atoms also change with their environment (for example, under the influence of electro-magnetic fields, etc.) without upsetting the laws of physics, but in accordance with these laws. Besides, the significance of the alleged changes of human nature is very hard to assess.

We now turn to the historicist contention that in the social sciences we must never assume that we have discovered a truly universal law since we cannot be sure whether its validity extends beyond the periods in which we have observed it to hold. This contention may be admitted, but only to the extent to which it holds good for the natural sciences also. In the natural sciences, it is clear, we can never be quite certain whether our laws are really universally valid, or whether they hold only in a certain period (for example, perhaps, only in the period in which the universe expands) or only in a certain region (perhaps in a region of comparatively weak gravitational fields). In spite of the impossibility of making sure of their universal validity, we do not add in our formulation of natural laws a condition saying that they are asserted only for the period for which they have been observed to hold or perhaps only within " the present cosmological period ". It would not be a sign of laudable scientific caution if we should add such a condition,[5] but a sign that we do not understand scientific procedure. For it is an important postulate of scientific method that we should search for laws with an unlimited realm of validity.[6] If we were to admit laws that are themselves subject to change, change could never be explained by law. (It would be the admission of miraculous change.) And it would be the end of scientific progress; for if unexpected observations were made, there would be no need to revise our theories; an *ad hoc* hypothesis of a change in the laws would "explain" everything.

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[3] Kepler's laws are chosen by Mill, for the reason that they are not general laws of motion but only (approximate) laws of planetary motion, as examples of what he calls, following Bacon, " *axiomata media* ". Cp. *Logic*, Book VI, Ch. V, § 5. Analogous *axiomata media* of a social science would be laws which hold for all " social systems " *of a certain kind*, rather than the more accidental regularities of a historically given period. The latter might be compared, not with Kepler's laws, but, for example, with regularities in the order of the planets of our particular Solar system.

[4] K. Mannheim, *op. cit.*, p. 177, introduces the expression " *principia media* " with reference to Mill (who speaks of *axiomata media*; cp. note 3 above) in order to denote what I have called " generalisations confined to the concrete historical period in which the relevant observations were made "; cp. for example, his passage (*op. cit.*, p. 178; see note 2, above): " The layman who observes the social world intelligently understands events primarily by the unconscious use of such *principia media* ", which are ". . . particular principles which obtain only in a certain epoch ". (Mannheim defines, *loc. cit.*, his *principia media* by saying that they are " in the last analysis universal forces in a concrete setting as they become integrated out of the various factors at work in a given place at a given time—a particular combination of circumstances which may never be repeated ".) Mannheim states that he does not follow " historicism, Hegelianism, and Marxism " in their failure to take " universal factors into account " (*op. cit.*, p. 177f.), so that his position is one of insisting on the importance of generalisations confined to concrete or individual historical periods, while admitting that we may proceed from them, by a " method of abstraction ", to " the general principles which are contained in them ". (As opposed to this view, I do not believe that the more general theories can be obtained by abstraction from those regularities of habits, legal procedures, etc., which, according to the examples given by Mannheim on pp. 179ff., constitute his *principia media.*)

[5] The suggestion has often been made that instead of vainly attempting to follow in sociology the example of physics, and to find universal sociological laws, we should rather follow in physics the example of a historicist sociology, and copy its laws which are limited to historical periods. Historicists who are anxious to emphasise the unity of physics and sociology are especially inclined to think on such lines. Cp. Neurath, *Erkenntnis*, Vol. VI, p. 399.

These arguments hold for the social sciences no less than for the natural sciences.

With this I conclude my criticism of the more fundamental among the anti-naturalistic claims of historicism. Before proceeding to discuss some of the less fundamental of these claims, I shall turn next to the most characteristic of its pro-naturalistic doctrines, namely, that we should search for the laws of historical developments.

(*To be concluded.*)

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[6] It is the same postulate which in physics leads, for example, to the demand that the red shifts observed in distant nebulæ should be *explained*; for without this postulate, it would be quite sufficient to assume that the laws of atomic frequency change with the different regions of the universe. And it is the same postulate which leads the theory of relativity to express the laws of motion, such as the law of addition of velocities, etc., uniformly for high and for low velocities (or for strong and weak gravitational fields) and to be dissatisfied with particular assumptions for different realms of velocity (or of gravitation). For a discussion of this postulate of the "Invariance of Natural Laws", and its opposition to that of the "Uniformity of Nature", see my *Logik der Forschung*, pp. 186ff. (section 79).

Commodity Stocks and the Trade Cycle: 1. A Note

By HARRY NORRIS

WHILST appreciating the usefulness of his putting a case before economists in relation to stock valuation and accounting usage, I think it proper for another accountant to comment on Mr. Lacey's criticisms of existing practice.[1] There is more logic in this practice than he would have us think.

Mr. Lacey advocates " Lifo " valuation, a method not unknown but infrequently used in this country. It contrasts with the more popular " first-in-first-out " basis. In relation to National Income statistics, the Lifo method gives figures of profits which need only a simple cost-of-living index deflation to give national " real income ", whilst the commoner method involves preliminary adjustments to exclude from income any accretion in stock values not related to an increase in quantities held (conversely for a decrease).[2] But whilst " real income " and " real capital " have meaning in national statistics, it seems doubtful if they have any useful significance in relation to a particular business. National statistics lead up to real income because only in terms of commodity flow can comparisons (international or temporal) be made—money is a mere comparative unit of expression in deflated income figures. But the accounts of a business undertaking are designed to reveal the *money* fruition which has arisen from operations during a period. This fruition, or profit, is the surplus of money revenue from the sale of goods (or supply of services) over the cost of supplying those goods (or services). And since the gross revenue could still have been earned if purchases had been curtailed by so much as to allow the stock to run down to nil (i.e., without cutting into the business's ability to meet orders placed by customers) then the expenditure to be eliminated (in the form of stock inventory, i.e., purchases carried forward) from chargeable costs is simply that comprised in these " curtailable " purchases—which correspond to physical stocks on hand. This is exactly what the " first-in-first-out " method achieves.

The basis of the accountant's method of profit measurement is the linking of revenues with relevant costs. Profit is the surplus of earnings over costs incurred and is not based on maintenance of capital either in a " real " or a " financial " sense. In this, I differ from the view expressed by Mr. Lacey at the foot of page 13. Whilst concepts of " real capital " and " real income " are useful in certain contexts, they are connected with physical volumes, not with money; I do not see that " real value " and " real worth " are admissible expressions. Mr. Lacey's attempt to translate the physical notions " real capital " and " real income " into monetary equivalents (thus, " real value of business capital ") is, I suggest, invalid. If, owing to rising prices, it costs more in one year to maintain the stock of article *A* at 1,000 units than it did to acquire the original 1,000, then the accounts must recognise this fact. The castigation of the businessman and/or his accountant for " financial self-deception " in recording an increase in the figure of " replaced stock " is unmerited.

Harking back to the days when trading consisted of discrete ventures, let us suppose that we wished to ascertain a trader's profit for a year ending on a date at which he had a shipment of spices and carpets *en route* from Venice to London. To do this we should aggregate the profits on all the ventures completed during the year (of course we are dealing with the days when account-keeping did not go beyond the separate-venture viewpoint). No account would, or should, be taken of any increases or decreases in the cost prices of the carpets and spices in transit as compared with those of a similar shipment on the high seas a year ago. We may think of the transactions of a present-day business simply as a large number of discrete ventures. The circumstance that multitudinous transactions overlap and that particular production factors contribute services in relation to a number of transactions merely adds to the complexity of accounting procedure; it does not provide a reason for departing from the actual-cost-of-stock valuation basis. Maintenance of real capital has no relevance in profit ascertainment. An advance in prices involves the entrepreneur in having to invest additional money if he wishes to maintain the same physical scale of activity (and perhaps in order to maintain his own standard of living). The investment required of two entrepreneurs doing an equal volume of business is the same (*cet. par.*) though one is just setting up his business (and whose stock must inevitably be valued at recent—higher—prices) whilst the other has been operating for some time and happened to have an equal (physical) stock a year ago, at that time costing less money. The additional financial investment of the established business is in no sense a charge in calculating its profits.

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[1] ECONOMICA, Feb., 1944.

[2] ECONOMICA, Nov., 1942, T. Barna.

If the entrepreneur were to calculate his profits in terms of maintaining capital intact, then nothing less than the aggregate value of his business would have significance. In accountancy language, we would have to provide for any variation in value of goodwill and not merely in that of the tangible assets. Thus a business producing a current yield of £5,000 in a year may fall in total value from £60,000 to £40,000 in the same period. There is a sense in which £15,000 has been lost. However, we draw a line between capital and revenue profits in the sphere of accountancy, and it is current yield—trading profit—which we are concerned to ascertain. (For one thing, total value is not subject to appraisal with sufficient precision to commend it to accountants for annual accounting treatment.) The notion of real capital seems to me to be without importance in relation to the individual business, to which only aggregate financial capital could be of interest.

The difference between the national position and the individual business's affairs is illustrated by reference to capital value which, for the latter, includes goodwill. According as a larger share of the social product goes to capital, so will the aggregate of business capital figures be higher, by reason of high goodwill values, irrespective of the size of the country's real capital. The existence of monopoly profits adds to business capital values but not to national capital. Whether Mr. Lacey likes it or not the accounting system must fit the economic facts of commerce, industry, and finance. Truth is best served in the long run by keeping to it rigidly rather than by telling oneself untruths, even though those untruths may cancel out or offset herd fears or hopes. One stands less immediate risk of being eaten by lions if one walks through a jungle blindfold—for an undetected lion will not involve that shewing of fear which often provokes attack. But the marauding lion will eat *some* men, even blindfolded ones, and the best course is to go forth keen-eyed and armed, on the look-out for lions. An immediate risk is worth while so that the eating may be stopped.

Mr. Lacey does not discuss in any detail the logic on which methods of stock valuation should be based. These methods cannot be settled without reference to the nature and significance of the measurement of the profits of individual businesses. To argue back from economic effects may be a useful part of a criticism of our economic system; it cannot lead us to accounting principles. For the cyclical character of trade will continue to exist whether or not the accounting treatment of stock inventories is amended. The remedy for the evil of fluctuations must be found in other fields. Nevertheless, the significance of what business men believe to be their profits, distinguished from what they really are, is very considerable. Opinion, in waves of optimism and pessimism, operates as an aggravation of incipient economic disequilibria. It is possible that if we could counter an upswing of trade with pessimistic accounting, and a downswing with optimistic figures, we might mitigate considerably the amplitude of the cycle. Thus far I am with Mr. Lacey in his view that Lifo stock valuation would make for stability (though the necessary amount of optimism or pessimism is not so readily measurable as to be equated with inventory-price variations). But is this an argument for that method? I suggest that it is not. Lifo is either right or wrong on other grounds; if wrong, then the evil effects consequent on its use are indeed an argument for the urgency of a change of practice. Mr. Lacey's declaration that " accountancy, like finance, must be our servant and not our master ", is a plausible catch phrase lending support to a dangerous doctrine of apparent expediency. But rather than seemingly-convenient arbitrary rules, we should use accounting methods based on logical principles of measurement.

In his desire to carry his point, it seems probable that Mr. Lacey misjudges the effect of accounting practice on the shewing of profits. Rather than magnifying them in " upgrade " years, and shrinking them in " downgrade " years, it is probable that the doctrine of smoothing profits is the dominating factor in the published accounts of public companies. A great number of large concerns maintain a rate of dividend with a much smaller range of variation than their true (but generally concealed) profits.

It is hard to understand why such a gap as lies between economic and accounting thought should exist at the present time when the respective fields of study overlap to a considerable degree. Though I have ventured to differ from Mr. Lacey in relation to the measurement of profits, I must commend his attempts to link accounting and economics in this way. Whilst economists and accountants may use different measures of capital and income for their several purposes, it is imperative that they should exchange views on methods of calculation so as to achieve a common conception of the nature of the terms they both employ. An interchange of opinions on the nature of profits alone would undoubtedly reveal much that would be useful to students and practitioners in both fields. Such co-operation is long overdue.

2. A Reply

By K. LACEY

MR. NORRIS is under a misapprehension if he thinks I doubt the logic of the present accounting system as applied to commodity stocks. Based on the premise of measuring the money fruition during the accounting period, it is utterly logical. Existing accounting methods do not, however, apply the principle of money fruition to other types of assets. The value of goodwill (if any) is left unchanged. The original cost of fixed assets is written off over their expected life irrespective of current realisable values of those assets. It is only in the case of commodity stocks that the present system is concerned with money fruition and in using this term Mr. Norris implies that in this respect the aim is that of keeping the financial capital intact, because money fruition is measured by the excess of closing money values of assets over opening money values of assets, subject to the conventional exceptions in the case of intangibles and fixed capital. The method of computing trading profits as described by Mr. Norris involves implications as to the method of drawing up the Balance Sheet; and when he states that it is the difference between the cost of the goods sold and their sales proceeds which represents the true profit he implies that the correct value of closing stocks is the cost of the particular parcels in stock assuming deliveries to have been made out of the earliest acquisitions. Under this method the cost of closing stocks may be higher than the cost of similar items in opening stocks, and if the trader is not dissatisfied with this variation of unit values it must be because the higher cost is somewhere near to the current realisable value of the stocks, i.e., he *is* concerned with the financial value of his capital. This contention is supported by the existing principle of stock valuation, " cost or market value, whichever is lower ", and when market values fall stocks are immediately written down. It is difficult to see how it can be argued that the basis or implication of this method is not that of keeping the financial capital intact.

I agree then with Mr. Norris that the present system is concerned with money fruition and have tried to demonstrate that this term implies keeping the financial capital intact as far as commodity stocks are concerned. Where I differ from Mr. Norris is in regarding this premise as sound. Which is the more important for us to know: our money fruition during the year, or the value of the increase of our real (quantitative) fruition? In considering the scientific accuracy of these alternative premises, it is not the logical superstructure which is at issue but the approximation of the two basic assumptions to reality. If we agree that with an increase in the quantity of goods we are better off (so long as we do not prevent ourselves from benefiting from our good fortune) then it follows that it is the quantitative increase, valued at current cost, which should form the basis of our income calculations, and not the money fruition. However logical the superstructure, it is not scientifically accurate to use as a basis of our income calculations a money fruition which gives results which are the reverse of reality. It is absurd to base our calculations upon the current costs of total stocks thus involving a fall in aggregate stock values and a reduction of income at a time when quantities have increased, thus:—

Opening stock 10,000 units at (say) £10 = £100,000
Closing stock 12,000 units at (say) £8 = £ 96,000

In doing so we are, in the above example, subtracting £20,000 from the value of our quantitative income by writing down replaced stocks from £10 to £8. It is only invalid to use a combination of quantity measurements and values if we accept Mr. Norris' own premise, namely that we are concerned solely with money fruition. If Mr. Norris' premise is rejected there is no reason why commodity stocks, like intangibles and fixed assets, should not be valued on a conventional basis (as opposed to a liquidity basis) which is nevertheless nearer to reality.

Mr. Norris reveals his bias for abstract financial values when he confuses my reference to the " real capital " of a business with its selling price as a going concern. The real worth at issue here is the quantity of fixed and floating assets, which I maintain should form the basis of our income computations. He also asks me to look at a number of financial facts whereas my article was asking him to look at concrete assets. It is necessary for practical purposes to record our real position in abstract financial terms, but such a record should be an abstract of the real position and not a pure abstraction such as is involved in the conception of money fruition. The absurdity of the idea of money fruition alone is revealed if we consider the extreme position during the period of mark inflation in Germany. It may seem laudable to us when Dr. Temple tells us (Speech to the Bank Officers' Guild, February 4th, 1943) of his tailor who sold him a suit on the basis of the cost of the cloth he had in stock despite the fact that cloth costs had risen considerably—that in fact is the logic of money fruition. But if prices had been rising, as in Germany, at a tremendous rate, the tailor would have been compelled to throw overboard the conception of money fruition and to consider his quantitative fruition, i.e., the purchasing power of his sales proceeds in replacing his cloth and in providing him with the necessities of life.

There is a measure of clash between the individual outlook and the social outlook and it arises because the individual tends to be perverted by abstract money values. My complaint is that our present accounting system helps to perpetuate and accentuate that perversion by having regard only to money fruition of commodity stocks. The perversion is not likely to go so far in the case of the small individual trader because he cannot so easily borrow to finance an increase in the replacement cost of his stocks. But it can and does tend to go to extremes in the case of larger businesses, which have no difficulty in financing their stocks at high values. Mr. Norris evinces a curious desire to escape from his own premise by arguing that many large businesses do in fact smooth out the flow of profits, or at least the flow of dividends, presumably so as to finance the movement in their own unit stock values. I wish it were wholly true, but I do not understand why Mr. Norris thinks it commendable if the present system shows the correct position. Actually the following figures do not seem to bear him out:—

	Corporate Reserves (White Paper) Mill £	Variation in value of replaced stocks (Mr. Barna) Mill £
1938	170	− 95
1939	180	+185
1940	190	+415
1941	195	+150

It will be seen that in a year (1938) in which stock values fell and when if anything there should have been a release of reserves, such reserves amounted to almost as much as in 1941 when values of replaced stocks increased by mill £415. There is no smoothing here, and reserves were obviously excessive in 1938 and inadequate in 1941.

We have, somehow, to build a bridge, albeit an imperfect one, between the " discrete venture " conception of Mr. Norris' example and the " continuing venture " conception inherent in all our social ideas, our full employment policy and our national income statistics. Money fruition gives a perfect logical superstructure because of its sheer abstraction, but if its premises are unsound (as I maintain), we should not be put off because the superstructure we can build upon the correct premises would be less perfect. We have a sufficient volume of continuing business to make it well worth while to correct our present methods. Moreover many of the principles of our present accounting system are built upon the assumption of a continuing business. The accounts of partnerships and companies continue without a break despite the death of partners and shareholders. The existence of a value for goodwill in a firm's Balance Sheet does not imply that the business is a " discrete venture "; nor can a mineowner or factory owner regard his business as coming within this category. Such businesses are by their very nature semi-permanent, and there is no compelling reason why commodity stocks should be valued on a basis of their realisable value, as if the business were in process of being closed down, when other assets are valued without regard to their market price. Mr. Norris' example of a trader merely liquidating his stocks is invalid as far as the conception of a continuing business is concerned; it is certainly not in line with the actual day-to-day position of the average trader. Where a business is being liquidated the accountant is obviously compelled to have regard to the realisable value of all assets and not only of commodity stocks.

Although a system of measuring income by reference to a quantitative increase of stocks has its apparent anomalies, such as when a new trader holds similar stock quantities as an existing trader but values them in his books at a higher price, nevertheless it has a logic of its own. Money, when first invested in stocks, ceases to become an abstract asset (cash) and becomes under this system a real asset (stocks); consequently a variation in the book value of identical units of stock in different traders' books is a reflection of the variation of the price level in the years in which the moneys were first invested. It is similar to the variation, arising out of fluctuations in wage rates and prices of supplies, in the cost of digging similar coal mines in different years. We have long been accustomed to the idea of the entrepreneur as one who takes the financial risk of profit or loss on income account; if we are to be in line with reality he must, because he is the owner-custodian of the real assets of the community, also take a similar financial risk on capital account and be content, in computing his selling prices and his income, with maintaining intact the quantities of assets acquired with his original investment. Such a system will not, as Mr. Norris assumes, create a difference between traders in the basis of the charge against profits in respect of the cost of the goods sold; in all cases, new and old traders alike, the basis will be replacement cost.

Let me put the argument in Mr. Norris' own language. When prices are rising a large slice of profits as now computed is needed to finance the additional investment involved in the increased replacement cost of stocks. And when prices fall, not only are all profits represented by cash and hence distributable, but some of the reserves previously made are no longer needed to finance replacement. If that is the position, then why, on rising prices, do accounts show this slice of profits as available and thus tempt directors to distribute it and speculators to push up share quotations to absurd heights, with disastrous results? Why not, like the French, say " hands off " to both shareholder and tax gatherer? And when prices fall, why do accounts show only the profits of the year as now computed and thus tempt directors to put aside the usual reserves and speculators to force down share quotations, with even more disastrous results? Why not, again like the French, include the reserves no longer required and make them available to shareholder and tax gatherer? If we regard variations in values of replaced stocks as investment at all, we ought to distinguish it from other types of investment because it does not represent an upward movement in the real (stocks) or potential (equipment, etc.) income trend—rather the reverse. Consequently such variations should be financed by the owner out of his profits as now computed; they should not be financed out of existing savings of the owner, i.e., by a reduction of business cash, nor out of existing savings of others, i.e., by borrowing.

I must not argue all over again why I think the present procedure introduces cumulative inflationary and deflationary effects upon our system. I have touched upon it incidentally above, and I doubt whether economists will dispute it. All I would add is that, in my opinion, it is evidence of an error in the implicit premises upon which the logic of our accounting system has been built.

Finally I would like to state that no slur is intended upon accountants and business men. But it is permissible for me to express the opinion that the " real capital " conception is more sensible (which is not the same as saying more logical) than the financial conceptions of business men and accountants, and in doing so I must explain why I hold that opinion.

Book Reviews

Iron and Steel in Britain 1870—1930. By T. H. Burnham and G. O. Hoskins. George Allen and Unwin. 1943. 352 pp. 25s.

In this book an engineer and metallurgist collaborates with an economist in an attempt to unravel the causes of the relative decline of the British iron and steel industry since the palmy days of the last century. The authors are aware of the confusion which sometimes arises from a failure to distinguish clearly between relative and absolute changes. It is unlikely that a country which takes the first steps in developing any industry can continue indefinitely to occupy the position of relative superiority then built up, so that " on general grounds it is reasonable to expect a relative decline in the British iron and steel industry ". Nevertheless in some contexts they still appear to expect the maintenance of historical relationships which on general grounds one might more reasonably expect to change, as when they argue that " had the competitive position of the iron and steel industry not been inferior it would be expected that their exports would have expanded *pro rata* with other exports, save to the extent that protectionism abroad would be more concerned with assisting native steel as opposed to other industries ".

The authors analyse in turn the various factors which might be suggested as causes of the decline. Apart from the inevitable effect of the growth of other industrial countries, it is argued that external factors were not of the first order of importance. The evil effects of post-war currency overvaluation, though " a most potent factor in retarding the development of the industry in the latter part of the post-war decade ", can easily be exaggerated, and it is argued that tariffs were not necessary for the maintenance of a prosperous industry. There was nothing seriously wrong with the fuel situation in Britain, and there is no *prima facie* evidence of a natural disadvantage in regard to raw materials. There does not appear to be much evidence to indicate that the industry suffered much handicap in the supply of capital up to 1913, and any restrictive tendencies of trade unions are not to be regarded as a major factor in the relative deterioration. Evidence is produced, on the other hand, " of long-standing neglect of plant development and organisation in anticipation of the trend of pig-iron making ", and in the steel industry of a falling behind " in planning on a large scale, in integrated and balanced plants, in mechanisation and organisation to give a high output in relation to the working force ". The developments which occurred at Corby before the war should have been begun forty years earlier, and the general picture presented to us is one of entrepreneurs failing to perform their functions efficiently. " The industry was slowly penetrated by science and failed to appreciate that it was entering a new highly technical phase." " The sense of security from inherited wealth explains an inherent conservatism, and a marked tendency to retain aged directors led to a love of compromise, a hesitant development of policy and opposition to change except when driven."

The non-expert reader inevitably has some difficulty in checking all the arguments presented, but the authors certainly make out a good *prima facie* case. It is, however, perhaps beyond the range of human ingenuity to trace out all the winding mazes of the history of the iron and steel industry in a story which by its clarity and comprehensiveness will at once command assent. The authors of this book have put together a vast amount of factual material, bearing both on British and on other iron and steel industries, but much of it is left in a somewhat indigestible shape, and a tendency to tedious repetition does little to facilitate the process of digestion. The book is worth reading, but the reader must be prepared for some strenuous effort, and at the end will probably feel that the last word has not yet been said on this intricate and intractable subject.

Allan G. B. Fisher.

Birth, Poverty and Wealth. By Richard M. Titmuss. Hamish Hamilton. London. 1943. 118 pp. 7s. 6d.

This book is almost solely concerned with infant mortality, its diminution during two decades, and its incidence on different social or economic classes. The annexed tables, compiled from those given by Mr. Titmuss, show sufficiently the basis of his argument. His statistics are limited to the three dates 1911, 1921–3, 1930–2, since it is only the records for these years that afford materials for his analyses.

The most striking result in the table reproduced is the general halving of infant mortality in 20 years in England and Wales ; in fact the fall has continued and the figure for 1939 is 50. But we should not be contented with so high a rate—in Class I in England and Wales, it was 33, and in some countries lower numbers have been reported.

The records are analysed by social classes (the Registrar-General's rather rough grading), by age at death, by cause of death, and by locality. In every case the incidence is the more severe the lower the social, or perhaps more accurately, the economic class. But the differences are much more conspicuous after the first month of life. Deaths within a month of birth are principally congenital or from other causes in Group A, and the reduction in the 20 years was much the same in all the Classes. Deaths in later months were largely due to infectious diseases and others that may be traced to overcrowding, poverty or ignorance. In these months the rate has been greatly reduced in all Classes, but it still becomes very much more severe as we go down the economic scale.

Here is the gravamen of Mr. Titmuss' criticism of the social organisation. Waste of infant life is associated with poverty. He does not explicity suggest remedies. It may be suggested that poverty, in its ordinary sense of lack of the means of livelihood, is not now prevalent, and it is rather in the association of crowding, ignorance, and incomplete development of social services with narrow means than in actual income, that we may find the remedial causes of the waste of life.

Mr. Titmuss turns rather a blind eye to the great and widespread improvement that has taken place, and emphasises what he regards as, so far, a permanent inequality between classes. He would consider, for example, a fall from 18·3 to 3·6 (rates for Class I, ages 6–12 months) to be much greater than one from 50·0 to 19·4 (Class V). The difference between the classes, it is true, is relatively greater, but it is absolutely less.

The reader may be left to find for himself the wealth of detail and of skilful and accurate analysis that form the basis of this important and timely study.

Infant Mortality By Social Class and Age.

Deaths from all Causes per 1,000 legitimate live births.
England and Wales.

Age		Under 1 month	1–3 months	3–6 months	6–12 months	Total
			1911.			
Class I	..	30·2	14·9	13·0	18·3	76
II	..	36·5	20·6	20·3	29·0	106
III	..	36·8	21·2	22·1	32·6	113
IV	..	38·6	22·7	23·8	36·4	122
V	..	42·5	28·6	31·4	50·0	153
All ..	..	39·1	23·3	24·9	37·5	125
			1930–2			
Class I	..	21·7	4·3	3·1	3·6	33
II	..	27·2	6·5	4·7	6·6	45
III	..	29·4	9·3	7·5	11·3	58
IV	..	31·9	10·8	9·4	14·7	67
V	..	32·5	13·1	12·0	19·4	77
All ..	..	30·2	10·0	8·4	13·0	62

Infant Mortality in England and Wales.

Causes of Mortality Under 1 Year
1930–2

		Group A	Group B	Rest	Total
Class I ..	..	20·5	6·2	6·0	33
V ..	..	33·1	34·3	9·7	77

Group A.—Premature births, Congenital malformations, Injury at birth, Convulsions, Congenital debility.

Group B.—Measles, Whooping cough, Diarrhœa and Enteritis, Tubercular diseases, Bronchitis and Pneumonia, Other infections and parasitic diseases.

Class I—Middle and upper classes.
Class II—Intermediate, comprising elements of I and II.
Class III—Skilled Labour.
Class IV—Intermediate, comprising elements of III and V.
Class V—Unskilled labour.

Miners, textile workers and agricultural workers excluded in 1911.

A. L. Bowley.

Report of the Foodgrains Policy Committee. (*India.*) 1943. Chairman: Sir Theodore Gregory. Delhi. 1943. 160 pp.

As an analysis of the food situation in India, of the main causes of the recent food crisis, of the reasons for the failure of the measures adopted up to the summer of 1943, and of the policy necessary to prevent the recurrence of famine, the Gregory Report is exemplary. But it has one major defect. This defect is the date of issue. Not that the Committee lingered over its report.

On the contrary it is clear that the evidence was gathered and considered and the report written at almost breakneck speed. The defect lies in the date of appointment of the Committee. All the major facts and figures appertaining to the food situation and relevant to the conclusions drawn and recommendations made were available many months before the summer of 1943. No new factors of fundamental importance had altered the situation in 1943, and any economist could have foretold the failure of the piecemeal and quite inadequate measures adopted until then to assure the food supply, control the rise in food prices and prevent hoarding. Some of the longer-term measures recommended—for instance, the reforms in the methods of collecting agricultural statistics, and the need for more and better manuring—have, for many decades, been a commonplace amongst those concerned with the improvement of India's agriculture. The reader asks himself in amazement how it was that when the military situation involved increased demand for foodstuffs in India, when the transport situation made it obviously more difficult to secure additional supplies from abroad should harvests fail or in case of other "natural" disasters, and when the Japanese victories prevented the possibility of securing the rice which Burma has always hitherto been in a position to supply (and supply quickly), no steps were taken to survey the whole situation and make sure that India's "four hundred millions" should at least be in a position to obtain those daily handfuls of cereals upon which they depend—pitifully and meagrely—for their subsistence. In other words, substantially the same report could have been written not long after the loss of Burma to the Japanese, in which case—had the recommendations been implemented—the whole disastrous muddle, involving great loss of life and still more widespread distress, might have been avoided. Someone has blundered.

The main causes of the food crisis, according to the report, may be summarised as follows. There has been no deterioration of the productive situation for India as a whole, but there have been important factors affecting adversely both the supply and demand sides. Normal supplies were reduced by the Bengal cyclone of October, 1942, and the widespread disease in the Aman paddy crop of Western Bengal, whilst certain normally deficit areas (Bombay and Malabar Coast) were deprived of a disproportionately large part of their normal supplies by the cutting off of Burma imports. Demand was increased by war conditions, including increased per capita consumption by cultivators, wage earners with higher incomes than before, and recruits for the Forces. There was also an increased wartime demand both for personal and family "security" reasons, and owing to the combination of an adverse supply situation in Bengal with an adverse pyschological situation due to proximity to the war zone. In addition there was increased holding of stocks both by cultivators and for "investment" and "black market" reasons. Above all it must not be forgotten that (owing to the inelasticity of demand) disproportionate changes in the prices of foodstuffs accompany changes in the supply of foodstuffs. This explains why the significance of imports, which form only a small proportion of total production, has been underestimated.

Five types of remedial measures are recommended: i.e. (i) an increase in available supplies, by arranging for a net import of foodstuffs, a Central Foodgrains Reserve, and increased home production; (ii) an improvement in "procurement", by Provincial and State machinery (adequately supervised from the centre) and increased inducements to the cultivators to sell; (iii) improvements in distribution, including the extension of rationing; (iv) a check on rising food prices, by statutory price control; and (v) a general overhaul of the machinery of administration and a readjustment of the relations between the Provinces and the Centre. It is admitted that the principle of a Central Government Foodgrains monopoly is the only perfectly logical solution of the difficulties, but this principle is rejected (probably with good sense) on the ground that during the present crisis time is not available for the creation of the vast organisation that would be involved. The point is stressed that public opinion will not tolerate a "hands-off" attitude by the Centre, and hence that although procurement should remain primarily in Provincial (and State) hands, the Centre should retain the last word on price changes, allocations of supplies, management of the Central Foodgrains Reserve, the management of special assistance operations and details of administration. If these measures are fully and quickly implemented any repetition of famine conditions should be avoided.

A second defect of the report is that it contains no clear chronological account and explanation of the the development and failure of the foodgrains policy pursued up to the summer of 1943. Certain aspects of this policy are discussed, but the relations between the Centre and the Provinces, and between the Provinces themselves and the reasons for the adoption of certain attitudes in particular Provinces never clearly emerge. Remarkably little is also said about speculation and hoarding in foodgrains. Perhaps it was considered best to observe the principle of "least said, soonest mended" in these ticklish matters, but it seems unfortunate not only that the policies adopted in the past should have been so insufficient and ineffective but that no adequate explanation should be given of the obstacles encountered by the responsible authorities in their efforts to promulgate and enforce effective remedies.

VERA ANSTEY.

British Finance 1930—1940. By WALTER A. MORTON. University of Wisconsin Press. 1943. xii + 356 pp. $4.00.

The central thesis of this lucid and comprehensive study is somewhat astringent, if not indeed discouraging. Professor Morton does not believe that any of the expedients tried during the desperate years 1931—1933 directly alleviated, much less cured, the Slump. What did cure it does not come within his purview in this book, but reading between the lines we are driven to the somewhat fatalistic conclusion that all we were able to do to our fainting economy was to stand back and give it air until it revived of its own accord. This does not, however, prevent Professor Morton's book from being exceedingly stimulating, and there will be few readers whom its clear and factual analysis does not drive to reconsider some cherished opinions.

The first part of the book—which shares the well-known characteristic of All Gaul—is devoted to a survey of the causes of Great Britain's departure from gold. Banking policy, the Budget, the Balance of Payments, the maldistribution of gold, and exchange-price disequilibrium all in turn come in for critical survey. All this is excellent; the facts are marshalled and the psychological influences recaptured with insight and impartiality. I found particularly valuable the author's detailed examination of the complex politico-economic tangle which frustrated clear thinking and decisive action during the crucial months of 1931.

The change which, consciously or unconsciously, has since come over our habits of thought is clearly illuminated by the reminders, afforded particularly by the chapters on the maldistribution of gold and on exchange disequilibrium, of the delusions which we used to cherish about the functions of gold and the scope of automatic mechanisms. Students of the inter-War period, seeking to understand the interaction of problems and opinions during the turning-point, will find both set out here in illuminating juxtaposition.

Part II follows on from these preparatory chapters with a full-dress study of the changes in the value of the pound from 1931 to 1940, parallelled by surveys of the fluctuations in the value and output of gold, in the balance of payments, and in the volume and direction of international trade. From the first of these Professor Morton derives the conclusion that the depreciation of sterling had little lasting effect on British trade, and was not even a necessary precursor of cheap money. In this argument, which expressly challenges the more positive view taken by Professor Harris in his "Exchange Depreciation", I believe Professor Morton to be right; but the available data combine cause and effect in such inextricable confusion that final proof is impossible. In any case, the roots of the ineffectiveness of sterling depreciation probably lie in the contemporary depreciation of the whole sterling area; and a discussion limited, as is Professor Morton's, to British, French and American experience is insufficiently widespread to embrace this particular feature.

He has an easier task in showing that the American devaluation was misguided, and that the collapse of the gold bloc was due to a largely illusory fear of "over-valuation". But the statement in the Introduction to the volume, that "the British suspension was the real cause of the American and Continental devaluations" (p. 8) is not, I think, either established by Professor Morton's own more cautious later chapters, or inherently probable. Fundamentally, the British devaluation was one of a series—the progressive substitution of looser and less galling links with gold for those which had been inherited from a previous, more or less irrelevant, dispensation. Why should it be postulated that it was the British break in 1931, rather than the French in 1927 or the Australian in 1930, which was (in modern slang) the prototype devaluation?

It is, however, with Part III of Professor Morton's book that I am most disposed to quarrel. Here he treats of Britain's domestic efforts to stimulate recovery—cheap money, fiscal policy, and the revival of investment. The conclusion drawn is (p. 317) "that cheap money may have done a little good, and that it certainly did no harm, but it still was not a powerful force in the British recovery". There is, of course, a good deal to be said for this view, and Professor Morton says it with all due emphasis—though not, I think, quite as cogently or convincingly as he makes the less debatable points in his earlier chapters. What his argument amounts to is that neither in theory nor in experience are lower rates of interest sufficiently stimulating to play much part in a revival of trade—compared, say, with the state of public confidence and the demand for the products of industry. But it is notoriously hard to prove a negative, and the reader may be forgiven if he rises from a study of these last five chapters still somewhat sceptical.

Indeed, Professor Morton encourages this attitude, for he is at pains to show that the type of investment which led the way to recovery—housing, public utilities and so on—was precisely the type which cheap money most favoured. And the fact, apparently so damning, that recovery did not get under way until 1935 is explained by his own demonstration that the cheapness of money did not seep through into bank—or building society—loans until 1934/5. This is surely at least as acceptable an explanation as the author's own view, that investment waited upon an autonomous recovery of commercial profits, and not on the encouragement of cheap money. Incidentally, Professor Morton does not mention the possible Hobsonian effect, indirectly derived from cheap money, of the transfer of purchasing-power from the rentier to the recipient of social security payments, which is so marked a feature of the 1932–35 Budgets, and which may also have played some part in restoring an active demand for the products of industry.

Even, however, if Professor Morton obtains no more conclusive verdict than "Not Proven" for this final part of his thesis, he has left us in his debt for a survey of a revolutionary epoch which will be no less stimulating to the independent researcher than illuminating to the student. His book is well produced, and the statistical information adequate and well marshalled (even though some of the series used might perhaps have been more fully explained—as, for example, the difference between Clark's "total gross investment" (p. 300) and Bretherton's "total investment" (p. 330)). I have noted very few misprints, and none likely to mislead.

J. K. Horsefield.

The Economics of War. By Horst Mendershausen. New York, Prentice-Hall. Revised Edition, 1943. xiv + 390 pp. $3.35.

This is the most thorough and systematic text-book on War Economics which has yet come my way. It covers the full cycle of transformations both in time and scope. It deals with the preparations for war, the transitions to war, the adaptations for total war, and the restoration of peace. It deals also with men, money and materials. While severely practical, it contrives always to keep in sight the main theoretical issues involved.

Mr. Mendershausen's method is one of selective illustration, based on facts and figures drawn impartially from the experience of all the belligerents in this war and (where still relevant) the last. He picks his way skilfully amid the welter of *suppressio veri* and *suggestio falsi* with which propaganda and security restrictions encumber the path of contemporary investigation. Although not even the far greater frankness of the American administration provides details of some aspects of the road to total war, his book has the advantage of being written in a country where the secrecy mania is less intensively developed than it is over here. In consequence the proportions of Mr. Mendershausen's study are far better maintained than would be possible for an investigator relying on British statistics alone. The unfamiliarity, to English readers, of the American scene is more than counterbalanced by the book's far greater completeness.

Amongst the scores of topics discussed it is possible only to pick out two or three for mention; the personal bias of the reviewer must be allowed for in what follows. Chapters VI and VII are devoted to the machinery for guiding the flow of materials through industry, Chapter VI dealing with priorities and allocations, and Chapter VII with price control. Mr. Mendershausen starts with a clear description of the working of the American priority system in the present war. He shows how the grant of top priority to a flood of demands reduced the hoped-for orderly preference scale " into the trivial affirmation that everything was terribly urgent ". This led to systems of super-priorities piled above the previous highest gradings; which again broke down for lack of co-ordination between demands and possible supplies. In contrast to this chaos, he next describes the successful working of the British system of allocating outputs, and finally shows how the United States, through the Controlled Materials Plan (successor to the Production Requirements Plan) has adopted a similar system. Of the success of that system it is probably too soon, and certainly impossible for an unofficial observer, to judge. But meantime Mr. Mendershausen's explanations, here and elsewhere, enable even the uninitiated to follow the meaning and purpose of the shifting kaleidoscope of American Government agencies as they successively tackle such problems as these.

On the other hand, the ensuing chapter—on price control in the war industries—rather illustrates the dangers of his methods. Here the administrative jungle becomes so thick that the reader is prone to lose his way. Clearer emphasis on the fundamental issues, and on such subjects as the basis of government contracts and the possibilities of price-control by technical costing rather than accountancy checks would have been helpful. Price-control over civilian supplies is dealt with in a separate chapter on the " Contraction of the Civilian Sector ", which again ably summarises a wide range of Government activities. (Though there is a curious absence of reference to the black market.)

The two chapters on War Finance start with the considerable advantage of occupying no more than a due proportion of the full scale of the book—forty pages out of something like four hundred. Within this space Mr. Mendershausen contrives to discuss most of the practical difficulties of avoiding inflation. It is possible to differ from him in emphasis—for instance, the significance of wartime increases in the National Income seems to require greater development, and a " spendings tax " has perhaps less *net* advantages than he suggests—but the discussion is none the less sane and illuminating in its collocation of detail.

Amongst the many other topics covered are those grouped under the general headings of man-power; the expansion and conversion of industrial capacity; foreign trade in war; economic warfare; and the problems of demobilisation and reconstruction. On all these issues Mr. Mendershausen sets out in orderly fashion facts and inferences which are nowhere else so accessible, and which are indispensable for a full study of the case. Taken as a whole, and despite occasional *lacunæ*, the book is one which no teacher seeking to discuss and illustrate War Economics can afford to overlook.

J. K. Horsefield.

History of Macy's of New York, 1858–1919: Chapters in the Evolution of the Department Store. By Ralph M. Hower. *Harvard Studies in Business History*, No. VII. Harvard University Press. 1943. xxvii + 500 pp. $3.75.

This is the first historical study to be made of a single department store, as a contribution to the general history of retailing, and Professor Hower is to be congratulated on having produced a book which is both scholarly and readable. His material is taken almost entirely from the Macy archives, but it is not, as the preface explains, in any sense an official record: it is a history written by an outsider from inside the firm. Yet it is understandably rather partisan, for one cannot imagine a historian living for years among the papers of a world like Macy's without feeling—in early days—a part of the family, or—in later days—part of an exciting and efficient institution. So Macy undercutting, even when carried to malignant lengths, is presented as good business and good fun; but attempts by rivals to defend their interests—ranging from small tenants of buildings in the way of Macy expansion to the great American Publishers'

Association—are not so favourably portrayed. So such things as Macy's 'exaggeration' of factories owned in Europe, or their violation of signed price-agreements are glossed over. But this mild partisanship is perfectly obvious to the reader, who himself soon becomes wrapped up in the adventure-story of Macy's "fight for the right to sell cheaply".

One sees Macy's growing from a small family business, specialising in "fancy dry goods", to a corporation with publicly-owned shares, functioning as a large and complicated department store. It is fascinating to read as a detailed section of social history, and doubly fascinating in that the author does succeed in showing the store moving forward in the general march of New York and even of America as a whole. There is, by the way, a useful definition of a department store—which includes offering many 'free' services, catering principally to women, and performing many functions usually done by wholesalers and importers—and there is an interesting discussion of the origins of the department store, and the reasons for its rise in the United States and in Europe.

How many things which we now take absolutely for granted are of fairly recent origin! Macy's were, for example, almost alone in mid-nineteenth century America in having a one-price policy—that is to say, eliminating the commoner practices of bargaining, auctions, special discounts, and prices adjusted to the social status of the purchaser. And in 1854 they announced with pride the novelty that "All money taken in this establishment goes through the Cashier's hands by tickets, and mistakes are almost impossible." Their advertising policy, too, unique in that period, anticipates by decades the immense sums to be spent by big stores in the twentieth century. For real entertainment, the early Macy advertisements can be recommended. Here is a charming example which demands to be quoted, from Macy's New Year blurb in 1859:

> "Trade is good at Macy's, and they are as happy as clams at high water at Macy's. Go, all hands; go in together, and be happy at Macy's."

Macy's began as they meant to go on—dealing for cash only, selling at one price (and that the lowest), and using aggressive advertising methods. Their cash policy was only slightly modified in the twentieth century; their one-price policy and their use of advertising were soon in general use; their undercutting is a by-word to this day. Undercutting was perhaps the corner-stone of their policy, though as the American standard of living rose they found that cheapness alone was not enough to attract customers, and they were forced to offer one by one the amenities and "free" services which we now consider typical of the department store. In the early twentieth century Macy's became involved in various important legal cases about the right to re-sell other people's products at Macy's own price—all of which they won. Their problems in the future, Professor Hower thinks, may lie partly with the increase of Trade Unionism, but more probably with the threat of 'government formulation of business policies'.

Absorbing as the book is, it is with a slight recoil that the reader observes the reverence of even a research worker in the United States for business. "America's culture," Hower says unaffectedly, "is predominantly a business culture, and the department store has been one of the chief participants in its propagation"—as, for instance, in Americanising immigrants by showing them typical houses and clothes, and so providing both a standard and the incentive to work and save for it. While reading the book one shares the Macy enthusiasm for absorbing college graduates as executives, and thus bringing intelligence and taste into business: closing it, one remembers the despair of the "Chain-store Daisy" in the revue-song, who "studied hard and made the grade, and *Macy's* was the grade she made"! One cannot but be impressed by Macy's as a monument of efficient organisation; but one's European soul obstinately fails to share Professor Hower's view of the department store as a vital factor in civilisation. One can enjoy his book, however, and learn much from it, while retaining one's inherent prejudices, for well-presented information about social and economic phenomena is always valuable. One will, indeed, even look forward to seeing the second volume, which is to deal with the development of Macy's after 1919—though for a historian nothing can be so interesting as the pioneer days of the 'fifties and sixties when things were not *quite* so slick.

BERNICE HOLT-SMITH.

Western Prices Before 1861. A Study of the Cincinnati Market. *Harvard Economic Studies*, Vol. LXXIV, 1943. By THOMAS SENIOR BERRY. Harvard University Press, Cambridge, Mass. London: Humphrey Milford, Oxford University Press. xxi + 645 pp. 28s.

This book is more than the title modestly claims it to be. It is a kind of price history for a period of roughly eighty years, or, to be more precise, an economic history based on the development of prices centred on Cincinnati, which was, for part of that time, the most important town of the Middle West of America. This restriction to one community is by no means a disadvantage, should it appear as such. In fact, the value of this admirable price study is the greater as the author does not attemot to generalise or to claim that the local price movements of Cincinnati are representative of a wider area or for the Middle West as a whole. Historical price research in all countries is the more valuable the more it is limited to well-defined regions; the prices of commodities and other goods of various regions of the country may not differ greatly in modern times, but as late as in the first half of the 19th century the diversions of prices were still quite substantial though gradually levelling up with the improvement which took place in transport facilities. In the past, the behaviour of prices in various regions of the United States was strongly influenced by events of a political, economic and demographic nature, and most of all by transport developments, such as canals and railway connections. The author, well aware of these facts, presents us with data relevant to the development of the Cincinnati Market, as, for instance, the evolution of the river transportation and the development of river and overland freight rates.

In Part III of the book we find a history of Cincinnati currency and banking, of cyclical disturbances and their influence on prices. This chapter is divided into four periods: the first comprises the years 1780–1823. Towards the end of this period, 1820–22, depreciated paper money led to increasing barter trade after the Cincinnati Branch of the United States Bank was closed and the local currency, the Miami Exporting Company notes, experienced great depreciation. The author points out: "The confusion and unpredictability of the post-war boom left their mark upon the people of the West for several decades. Banks were detested by many members of the rural classes, and the Bank of the United States was treated with special antipathy because it collected its debts in 'good money' or foreclosed." In another connection, we are told that "Westerners felt more fear for banks than for thieves in the early twenties" (of the last century).

The next period, from 1823–1835, was one of comparative stability of Western currencies; this period also shows a marked stability of commodity prices. The interval of 1823–1834, Berry writes, provides a parallel to 1921–1930, so far as general price behaviour is concerned. Furthermore, one finds the same disharmony within the price structure in each instance, with the important difference that the Western prices of agricultural items were rising in one case and falling in the other.

In the period of 1835–1843 fell the crisis of 1836–1839 and the "panic" of 1837. Following the speculation of 1835–1836, the deflation of commodity prices of 1839–1843 is compared by Berry with that of 1819–1821 on the one hand and of 1929–1933 on the other.

The last period deals with 1843–1861. The prices and panic of 1854 (followed by a boom in 1855) was the forerunner of the world-wide crisis of 1857. The effect of the crisis of 1857, however, was comparatively not too severely felt in the Middle West as it was in New York and other parts of the United States. The author's general conclusions are that: "The settlement of the West did not, however, proceed at a rate which was even approximately constant. On the contrary, it tended to surge and recede in tides of fairly equal length (eighteen to twenty-two years). Roughly speaking, the interval between 1785 and 1861 witnessed four of these tides, the timing of which is most sharply defined by agricultural prices and the sales of public lands but is also characterised by the organisation and dissolution of banks, the loans, deposits, and circulation thereof, the expansion and contraction of debts at home and abroad by states and individuals, the immigration and trade of the nation, and several other important series of economic data. Unlike ocean tides, these movements possessed phases very unequal in duration . . . the flood phase averaged about sixteen years, the ebb only three or four years."

It is noteworthy that whereas agricultural prices showed no major trend, or any long wave between 1788–1850, manufactured and imported goods declined greatly during this period; thus, the Middle West experienced a great increase in purchasing power due to the different behaviour of prices of goods they produced and of those they imported.

The chapters on Community Prices, Production and Trade are very illuminating. The description of the Salt Market, for instance, offers remarkable instances how every trick of the trade was used to secure high prices. The Cincinnati Salt Agent, in 1853, "advised sub-agents not to load consumers up with salt while the water was high and foreign salt was plentiful. Rather, instructions were to wait until the rivers were low (this happened frequently just before packing season), since sales could then be concluded at higher prices without fear of competition from the foreign article". As to the relationship of the salt combinations to later trusts, we read that "the oil industry is known to have borrowed technical methods from the salt industry and it is surely probable that commercial methods were also transferred".

In the chapter on Weights and Measures, the author takes special care to explain that little change, on the whole, seems to have occurred in the standard weights. As Berry points out, his findings do not tally with Sir William Beveridge's statement that "as often as not, the mere mention of a weight or measure indicated a departure from traditional practice".[1]

The author's study confirms the outcome of research in European continental countries which led to the conclusion that, except when measures of capacity were turned into units of weight, changes in standard measures or weights were not frequent, and, when they occurred, in many cases sooner or later the former standard was introduced again.

The book is a most valuable contribution to price history and will be an indispensable source of information to students of American economic history.

M. J. Elsas.

The Native Labor Problem of South Africa. By J. N. Tinley. University of North Carolina Press. Humphrey Milford. 1942. xviii + 281 pp. 18s. 6d.

This book provides a useful introduction to South African labour conditions. Professor Tinley begins with a brief sketch of the economic and political background, which is, however, marred by a few inaccuracies both of detail and of historical perspective. For example, the author says that before the opening up of the diamond mines in the 1870's there was no labour problem, whereas, as he himself states elsewhere, slave labour was introduced at the very inception of the settlement at the Cape, and in the 1860's Natal resorted to the importation of indentured Indian labour for the sugar plantations.

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[1] *Prices and Wages in England from the 12th to the 19th Century*, 1939.

After enumerating the non-European groups, which constitute the main supply of "unskilled" labour, Professor Tinley describes those features of the social and cultural life of the preponderant Natives (Bantu) likely to affect their assimilation into the industrial and economic system of the Europeans. He then gives a rather summary account of the laws and regulations governing the employment of Natives in urban areas and on farms. From this résumé of the supply of non-European labour we pass to the demand. Tables, compiled chiefly from official publications, are used to show the location and output of different types of farming and the output, wages and number of employees in mining and manufacturing industry. With these figures as a basis Professor Tinley attempts to sum the total demand for Native labour and to compare it with the supply. He comes to the conclusion that, leaving out of consideration the possibility of a fall in the demand for gold, if pre-war trends continue "in time of boom there may be serious shortages in the total supply of labour". This is, I think, the weakest section of the book. Professor Tinley apparently accepts the contention that there was a "shortage" of labour in 1938 but he fails to relate it to wage rates, to the structure of the labour market, or to the centralised recruitment and engagement of labour in the important gold mining industry.

The author shows how the trade unions, aided and abetted by successive Governments and with the support of the great majority of the politically dominant Europeans, have been able by demanding high wages and by limiting entry into skilled trades, to perpetuate the relative scarcity of skilled labour and to keep it largely a European preserve. Natives, debarred from acquiring skill by a variety of factors—custom, the apprenticeship system, colour bars imposed both by legal regulation and trade union pressure, the high wage policy and rigid delimitation of work enforced by the trade unions—remain for the most part "at a dead level of low efficiency". Native wages are correspondingly low. In 1938 on the gold mines of the Witwatersrand the average wage, including the cost of food, accommodation and other services, was £50 for a full year's work. The average wage of Natives in manufacturing industries was lower (£44 a year in 1936/7), although there is much more variation in wages between different industries and establishments than in goldmining. Native wages vary from £33 per annum (including allowances for food and quarters) in the processing of raw materials to £62 in the clothing industry. In contrast the wages of Europeans in gold mining averaged £404 (1938) and in manufacturing industry £234.

The internal market remains small as a result of the low productivity and buying power of practically all the eight million non-Europeans and of that part of the European population which is not able to enter the professions, to obtain skilled work at trade union rates, or to find protected jobs in government and semi-official undertakings.

Professor Tinley is explicit in his condemnation of South African labour policy. "If South Africa does not abandon its highly artificial wage policies, it will be necessary for the Government to resort to more and more restrictions and more and more subvention of agriculture and industry until the whole economy is placed in an economic strait-jacket. Eventually, however, its whole economy will collapse if that country continues to pursue policies which appear to be in direct opposition to sound economic principles. Collapse has been prevented so far only because the country has been fortunate enough to have mineral resources for which the demand has been greatly though temporarily stimulated."

Professor Tinley thinks the abandonment of the present restrictive wage policy need not involve a lowering of the level of European wages. For Native wages might be raised as a result of the development of greater efficiency. He considers that so far it is largely European labour which has benefited from improved technique and that more attention will have to be given to the gradual lowering of prices so that the real wages of the lower income groups may be raised.

In the absence of comparative data and vital statistics of the non-European rural population, it is difficult to generalise, but responsible opinion, confirmed by recent surveys, considers that the great majority of the inhabitants of South Africa are undernourished and that the physical condition of the Native population is on the downgrade. The remedy lies in increasing efficiency and output, but in manufacturing industry wage policy tends to maintain and force costs up, while in agriculture the policy of placing the control of prices, marketing and even processing, in the hands of boards largely composed of the producers concerned, has resulted in price maintenance and, in the author's view, in "the heaviest burden falling upon those with the smallest incomes, namely, the Natives".

Most of the material for this book was collected during an eight months' visit in 1939 and 1940, but the pressure of the war has not forced South Africa to abandon her restrictive labour policy. Europeans have not been shaken out of their complacent acceptance of racial discrimination and all that it entails in the economic field. Here and there Natives are being employed on semi-skilled work in new and developing industries, but in spite of the extension of industrial production and complaints of "shortage" of labour, there has been no general attempt to train Natives to take over skilled work. It seems unlikely that trade unions, which have strenuously opposed dilution during the war, will lower their barriers to Natives after the war, when they will be faced with the problem of re-absorbing the artisans now in the armed forces.

With regard to agricultural policy, the war seems to have provided a heaven-sent opportunity for the extension of control and the raising of prices. In view of pre-war trends it appears very unlikely that control will be relinquished or that the adjustment of prices will be downwards.

Professor Tinley's remarks on general economic policy are worthy of attention. He exposes the shallowness of the belief that the economy can be propped by fostering manufacturing industries, unless these are based on efficient low cost production. Until the great mass of the population is raised to a higher earning capacity, the collapse of goldmining would destroy a large part of the small domestic market at present available for protected industries and, once wartime restrictions on transport have disappeared, local high cost industries have little chance of securing markets outside the Union.

South Africa's labour policy is important beyond the boundaries of the Union. Native labour is drawn from the territories to the north and Europeans (miners and settlers) migrate northwards. There is already a tendency for the labour policy of the Union to spread and to come into conflict with the ideals of colonial development in neighbouring territories. Those interested in the future of Africa will find in this book a thoughtful and well-informed survey of economic trends in South Africa, while within the Union itself, it is to be hoped that Professor Tinley's warnings will be heeded.

SHEILA T. VAN DER HORST.

Die Gesellschaftskrisis der Gegenwart. By WILHELM ROEPKE. Zürich (Eugen Rentsch Verlag). 1942. 410 pp.

This is a book to warm the heart of the student of Society. Living in an Age of Superstition has its compensations. It forces us to re-examine our beliefs, to see that they include no mere counter-superstitions, to make explicit what has been implicit, and to cast off ideas which are only historical hangers-on to our true system. This is what Professor Roepke tries—in the opinion of the reviewer, with much success—to do in this book.

His thesis is common enough among modern liberals. Some time in the 19th century the world went wrong. When the ferment of capitalism dissolved the old world the result was not the sweet, wholesome society that the liberals expected. The error of errors was the failure to provide the right legal framework for the competitive system. From this there followed its failure to satisfy even men of good will, and thus there came the rise of collectivist follies. The State of the future must therefore be a vigorous, interfering State, but its interferences must be subject to the Rule of Law and its aim must be to preserve and extend the competitive system.

So far Professor Roepke will gain the fullest assent of his liberal readers. He will also earn much gratitude for his skilled, eloquent and high-minded handling of the errors of socialism and of attempts to mix it with capitalism. He rightly stresses the essential difference between his style of building—the authentic Liberal style—and these baroque socialist-capitalist structures. However, his programme includes something which the English liberal instinctively thinks of as a superstition—the re-establishment of a strong peasant and small craftsman class. It requires a real effort for the English liberal to give this idea fair consideration, for historically he has always seen it in other men's camps, wrapped in some romantic anti-liberal clothing. Yet it does deserve his earnest attention. It is true that Roepke's treatment of it is open to attack. He bases much on the case, which may be a special case, of Switzerland. He gives insufficient attention to the fact that Germany, the centre and origin of the world-disease, was, not long ago, strong in peasantry and small craftsmen; that indeed in the days of depression it was Germany, not England or America, that produced the Randsiedler, whom he would praise so much. He does not explain how it is that if the Riesenstadt and the landless labourer are the symptoms of the most advanced disease, England remained healthy so long.

These objections, however, are not decisive. What Roepke is trying to find is the ultimate source of that healthy conservatism without the balance of which your radical becomes a society-breaker. It may be that England, not Switzerland, is the special case. If the fund of English steady conservatism ever runs out we may need to restock it from traditions that we long ago lost.

A. SHENFIELD.

Europe's Overseas Needs 1919–20, and How They Were Met. Economic, Financial and Transit Department of the League of Nations. Allen & Unwin. 1943. 52 pp. 2s. 6d.

This brief but effective report should be read in conjuction with both its companion study, entitled *Relief Deliveries and Relief Loans, 1919–23*, and the report on *The Transition from War to Peace Economy*, by the League's Delegation on Economic Depressions. It summarises the main features of the relief policy pursued after the last war, and explains convincingly how and why the policy, as a whole, failed so dismally.

The policy pursued was to supply food by official and unofficial relief organisations, but no parallel international plan was evolved to provide raw materials and other essential goods required to restore economic activity in war-affected areas. It was not until 1920 that the "ter Meulen" plan, to provide international credit to enable importers to secure raw materials and other primary necessities, was presented to the Brussels Conference. Even so, the plan was designed to secure (and assure) private commercial credit, and was to be in no way associated with the issue of international Government loans. The plan failed to materialise, and in any case came too late, and was not bold enough in its conception. The result was that the war-stricken and devastated States were forced to rely solely on their own meagre financial resources. This proved disastrous. It led directly to currency depreciation, a price boom and runaway inflation. This caused "a violent redistribution of national income and wealth and prepared the ground for social unrest and political agitation".

The collapse of currencies, which was inevitable in the absence of any international plan for the restoration of economic activity, necessitated action; but this took the form of "curing one festering spot after another", rather than of any general attempt to "restore health and vitality to the whole economic body of the continent".

A contrast is drawn with the successful League schemes for the financial reconstruction of Austria (1922), Germany (1924), Hungary (1924) and Poland (1927). These succeeded, temporarily, because they "assumed the interdependence of reconstruction, currency, currency stabilisation, budget equilibrium and capital import": i.e. they dealt with the problem as a whole. But they came too late to provide a permanent solution.

The moral is clear, and is worked out in greater detail in the report on "The Transition from War to Peace Economy". When the present war ends, it is essential that international action should be taken to provide the means—financial and in the form of goods—of restoring economic activity and providing employment in those countries which are not in a position to attract private capital and supplies.

VERA ANSTEY.

The Transition from War to Peace Economy. Part I of the Report of the Delegation on Economic Depressions of the League of Nations. Allen & Unwin. 1943. 118 pp. 4s. 6d.

This report discusses clearly and forcibly some of the most fundamental factors and principles which should be taken into consideration in planning post-war reconstruction during the transition from war to peace. A future volume will deal more fully with the longer-term problem. The report can be wholeheartedly recommended to the general public, as well as to all concerned with the formulation, propagation and administration of post-war economic plans. If the conclusions become generally accepted, this may provide a much-needed antidote to the present apparent tendency to departmentalise the formulation of international economic schemes.

The chief conclusion is that the essence of "the task before us" is to formulate our aims in a comprehensive and comprehensible manner, so that the policy devised may prove an integrated whole, capable of co-ordinating all the policies associated with the various aspects of post-war economic and social development—for instance, currency, finance, capital investment, full employment, education and technical training, and nutrition—in such a manner that steps taken in one direction may not neutralise actions in other spheres.

To the present writer there appear to be three formidable dangers confronting post-war planning. First, that the formulation of plans may be entrusted to experts at too early a stage, with the (possible) result that they may concentrate on technical details, and in so doing forget the ultimate objects of planning. Second, in consequence of this over-specialisation, that the plans will become incomprehensible to the public and hence removed from democratic control. Third, that the various schemes may fail to provide the elasticity which is essential if they are to prove capable of gradual evolution in consonance with changing circumstances and needs, and to prove acceptable and applicable to the very varied frameworks provided by individual members of the society of nations. The task of the experts is to provide the means of implementing the general policy agreed upon by the representatives of the peoples and accepted by the general public.

The Report, after an introduction which is partly historical, and partly a discussion of the objectives of economic policy and an analysis of the aims and scope of the following chapters, deals *seriatim* with the effects of war economy, the domestic issues raised by the transition from war to peace, the international problems involved, and the main conclusions which emerge.

The most vital chapter is that on the international problem. This discusses, in a manner not attempted in (for instance) the Keynes and White Currency Schemes, what is the relation between international currency and investment plans, and the more general problems of providing post-war relief, organising reconstruction, and securing full employment. After 1918, it is argued, the mistake was made of leaving reconstruction to private enterprise. This ignored the fact that it is precisely where risks are great and the hope of profits remote that assistance is most necessary. Relief, reconstruction and the maintenance of employment and prosperity are a continuous process. "Relief is the first step in reconstruction, and if it fails to reconstruct it is ineffective charity." If governments try to stabilise national price-levels, but permit unlimited competition for food and raw materials in the world market, their policy will necessarily fail. If governments adopt diverse credit policies, the chances of establishing an international price equilibrium may be indefinitely postponed, and without the latter it is impossible to secure full employment or to remove exchange control and trade restrictions. National stocks (for instance of food and raw materials) will become depleted, competition will force up prices, only financially strong nations will be able to obtain supplies, so that inflation and currency depreciation will become inevitable. International rationing and credit control and supply must be maintained so long as serious scarcity continues. Although it is impossible to lay down absolute criteria for international rationing, three negative criteria may be used as a guide to priorities: i.e. the mitigation of real social distress; the avoidance of inflation; and the avoidance of commitments likely to lead to a subsequent financial breakdown.

This is certainly one of the most valuable reports so far issued by the League, and its usefulness is greatly augmented by its lucidity.

VERA ANSTEY.

Taxing to Prevent Inflation. By Carl Shoup, Milton Friedman and Ruth P. Mack. Columbia University Press. Oxford University Press. 1943. xii + 236 pp. 18s. 6d.

This book contains four studies, on differing planes of particularity, in the related problems of taxation policy and the national income. It derives from an attempt to estimate the amount of additional taxation required to avert inflation through a defined future period, and is illustrated by examples referring to the American fiscal year 1941–42. Access to Treasury records, and discussions with officials responsible for the Government estimates, have facilitated enquiries which at least bring together what seem to be the major factors relevant to the problems studied. That this is not the same thing as solving these problems, the writers are fully aware, and criticism is disarmed by their own modest assessment of their results (p. 75): "Both in the Treasury and elsewhere the figure for tax requirements to avert inflation, so far as that aim was specified, was being reached by a process of individual intuition, strongly coloured by guesses of what Congress or the public at large would be willing to accept. Crude as are the steps followed in the present study, they are presumably superior to the intuitive process. There is at least somewhat more chance to discover the mistakes".

The sentences just quoted are from Part I, for which the three authors are collectively responsible, and which summarises their conclusions on the main problem. Starting with four alternative estimates of the increase in total output expected to accrue between October, 1941, and June, 1942, and progressively introducing about thirty refining factors, they reach the conclusion that the additional taxes necessary to prevent any rise in the price-level would have amounted to between 125 per cent. and 153 per cent. of the assumed increase in output, varying because the size of the latter itself affects certain of the subsequent refinements—especially the volume of resources diverted to an independently estimated expenditure on defence. In absolute sums the new taxation required was found to vary between $4,600 million and $8,400 million, according to the assumptions made. The wide range of these results provides, of course—as the authors say—its own answer to the question whether the method is yet developed to a point at which it can be used effectively to shape budget policy. It also indicates how far short even the lavish American statistics fall when compared with the minimum requirements for efficient control of the national income and thus of the trade cycle.

This latter aspect is dealt with in more detail by Miss Mack in Part IV, which is at once the most elaborate and most difficult section of the book. The latter quality derives partly from the extreme complexity of the subject, but partly, it must be said, from the style, which is extremely compressed and sometimes indulges in phrases of picturesque obscurity ("Answers become more difficult when it is not clear whether a machinery or construction boom will hop an inventory band wagon") (p. 197). The theme of this Part is an attempt to forecast month-to-month changes in the size of the National Income during the period June, 1941, to February, 1942, using figures which are illustrative of the method rather than exact calculations. The process of prophecy is found to involve six stages, during which upwards of fifty separate factors are independently assessed and fitted together to build up the final estimate. Every one of these factors involves forming a judgment of the weight and impact to be imputed to the force concerned; and Miss Mack tells us that she has since changed her mind about many of these detailed judgments, without specifying which. The reviewer may therefore confine his comments to an expression of respectful admiration for the fecundity and originality of Miss Mack's invention in this jig-saw of a project.

Fundamentally, the results set out in Part IV derive from two portmanteau conceptions—"consumers' ready purchase money" and "goods ready for sale", both of which are necessarily ex-ante entities. The interaction of these two shows the trend of economic activity. Emphasis is laid on the empirical deduction, with which in theory we are already familiar, that the turning-points of the trade cycle are to be expected, not at the moments when ex-ante savings and ex-ante investment attain equality, but at some earlier instants when the fluctuating gap between them reaches certain significant levels. Miss Mack makes a brave attempt to assess qualitatively the influences which determine these levels; but quantitatively they are at present unpredictable, not only because the basic data for the two concepts themselves is so hazy, but because there exists no body of knowledge about previous trade cycles from which could be drawn any deductions about the significant relations between them. Whether, in fact, statistical detail of the scope and accuracy required by this technique can ever be made available, at least in time to be of service, must remain in doubt; but that the signpost which Miss Mack has erected points down a promising road no one can question.

The other two parts of the book must be dealt with briefly. Part II, by Mr. Shoup, discusses the type of taxation best adapted to the purpose of controlling a potential inflation. Discarding as unsuitable and/or impracticable various unorthodox expedients, he reaches the familiar conclusion that a combination of income tax and sales-tax is best. To this, however, he adds the unfamiliar rider that the sales-tax should preferably be on retail sales. His arguments for this turn on the psychological advantage of keeping as low as possible the nominal rate of tax necessary to raise a given revenue (pp. 87–91); but the comparison of this advantage with the administrative difficulties of collecting the more widely diffused tax is obviously a matter of opinion, and Mr. Shoup is not entirely convincing in his choice.

Part III, by Mr. Friedman, analyses previous attempts, by Professor Angell, the "Iowa Group", Messrs. Gilbert and Perlo, and the British "inflationary gap" technique, to predict the onset of inflation; and finds that none of them has so far given trustworthy results. The shortcomings of each method are displayed, but Mr. Friedman is benevolently aware that in fields where luck and good judgment have to deputise for non-existent statistics, success is scarcely to be expected.

This is a book about an experiment—an attempt, as Miss Mack says, to devise a set of instruments to assist in the difficult craft of prediction. Everyone interested in the problems with which it deals will be well repaid for a study of its frank and ingenious analysis.

J. K. HORSEFIELD.

Earnings and Social Security in the United States. By W. S. WOYTINSKY. Committee on Social Security, Social Science Research Council, Washington, D.C. 1943. xiii + 260, 41 pp. $2.50.

This volume brings to an end a series no studies of the American labour market undertaken by Mr. Woytinsky for the Committee on Social Security. It does not give a full indication of the new material made available in the course of this war, since the manuscript was finished early in 1942, and its publication was delayed. But it serves its main purpose admirably, namely, to demonstrate the wealth of statistical sources made available through the working of the social security measures. The detailed tables on which most of the text is based are published separately.

The study opens with a chapter on methodology, explaining the procedure adopted in comparing various distributions and in measuring inequality. Extensive use is made of the technique of relative deciles and quartiles, but the author recognises that, for gauging inequality, this method is in certain ways inferior to other well-known methods evolved in mathematical statistics. Mr. Woytinsky shows skill and originality in his manipulation of the decile-quartile apparatus for comparing different distributions.

The primary data consisted of the national income statistics of the Department of Commerce and the figures of taxable earnings derived from the Social Security Board. Unfortunately the concept of income is not identical in the two sets of source material. In the national income figures the *family* is the unit, while the social security programme is based on *individual* earnings. Moreover, the Social Security Act does not cover certain industries which are included in the national income classification. To overcome these discrepancies, it was necessary to reproduce the distribution of national income by consumer units in terms of individual earnings and then to check this estimated distribution of individual earnings with the actual taxable wage returns derived from the Social Security Board. Mr. Woytinsky was able to show that " the actual structure of taxable wages is in perfect alignment with their theoretical distribution, if allowance is made for shifts between covered and non-covered occupations ".

Part III of the book gives a detailed analysis of the structure of wages in the United States, with special reference to differences by sex, race and age of the workers, industry, occupation and geographical region. Part IV includes an interesting survey of the long-run trend in income and wages per head. It is found that over the last hundred years there has been a more or less steady growth of the *per capita* wage by 1.0 to 2.0 per cent. annually. According to Mr. Woytinsky, " a widely held supposition that this upward trend has been retarded in the past three or four decades is by no means established by available information, and it is entirely possible that the economic system is still in a phase of rapid expansion ". Of particular interest to students of social security and family allowances are the concluding chapters on " Families as beneficiaries of old-age and survivors' insurance " and " Income cycles in the life of families and individuals ".

The results obtained in the study of American wages by sex of the workers are of special interest to English readers, now that the subject of " equal pay " is topical in this country. Industry by industry, the ratios of female to male earnings are much lower in Great Britain than in the United States. Mr. Woytinsky found that, as a rule, the smaller the proportion of women in the labour force of an industry, the higher the ratio of their earnings to those of men. His general conclusion is that " differences in pay rates and annual earnings by sex of workers are due to differences in the industrial and occupational distribution of male and female employees and in the continuity of their employment rather than to discrimination of employers against women ". He does not, however, inquire to what extent the existing industrial and occupational distribution of women workers is itself a result of discrimination.

Mr. Woytinsky has produced a workmanlike and comprehensive volume which deserves to be carefully studied by those interested in quantitative analysis of the labour market as well as by exponents of social security.

The trouble with this book is that too much has been packed into its 250 pages, with the result that one or two important problems are either left in mid-air or dismissed with an unconvincing generalisation. Take, for instance, the old problem of *explaining* the range of individual earnings. The chief factors which make up the explanation are the commonplace of every economics text-book ; but what we know less about is the relative weight to be assigned to these factors. The volume under review gives us valuable statistical *description* of some of the elements at work, but does not go far towards satisfying our desire for *interpretation.* One would also have liked to see included a section on the influence of trade union organisation on the structure of American wages.

BRINLEY THOMAS.

Agricultural Production in Continental Europe during the 1914–18 War and the Reconstruction Period. League of Nations, 1943. Published by Allen & Unwin. League of Nations Publications Department. 122 pp. 7s. 6d.

According to Mr. Loveday, the Director of the League Department responsible for this publication, " this volume is historical in form and practical in purpose ". It is certainly a most interesting and timely publication. Indeed, its usefulness is so obvious, that it seems surprising that, in spite of the numerous books published on the economics of the last war, a study of this kind was not undertaken earlier.

The main objects of the book are to bring together the existing data on production, trade and consumption of major foodstuffs in the regions most affected by the first world war during the war and in the early post-war period ; and, further, to explain in the light of these data the causes of the decline in food production during the war and of the slow rate of recovery after the war.

Owing to lack of material for some countries, the first object could be carried out only in relation to the production of cereals and potatoes while animal and other vegetable foodstuffs had to be left out of account. For several countries, however, notably France and Germany, the statistics are more complete and provide also information on the number and weight of livestock and on milk production.

It is impossible to enter in detail into the interesting results of the careful analysis which are set out lucidly in a great many tables, graphs and map diagrams. The outstanding facts are that by 1919 food production on the European continent had declined by one-third of the average of 1909–13, and that by 1925 recovery had not yet been fully achieved.

The decline in output during the war reduced the food standard of the European population, however, to a very varying degree. Apart from the somewhat divergent development between Western and Eastern Europe and differences between various regions of the same country, the food position of the town population, especially in Western and Central Europe, was much worse than it was in rural districts. " The starvation in Germany might have been greatly mitigated had the available food itself been better distributed." It is estimated, for instance, that the milk supply of Vienna dropped by 90 to 95 per cent. of the pre-war average.

Several factors contributed towards the decline in agricultural output during the war, but the most potent were the withdrawal of labour from agriculture and the shortage of fertilizers, particularly phosphates. The relatively slow rate of recovery, especially in cereal production, was due, first, to the marked deterioration of the productive apparatus and, second, to the economic and social disorganisation caused mainly by monetary inflation in Central Europe and by radical land reforms in Eastern Europe.

The concluding section draws a comparison, by necessity only tentatively, between the condition of European agriculture at the end of the last war, and the position in which the continent presumably will find itself when the present war comes to an end. The general conclusion is " that, fundamentally, the position of the European Continent is weaker at present than it was at the end of the last war, and is bound to weaken further the longer the war lasts ".

As this is mainly a statistical study, no attempt is made to draw any lessons with regard to future agricultural and general economic policy. But the political economist should not find it difficult to draw such lessons. The most general and, perhaps, also the most important is that " planned " help towards recovery should not aim at restoring the pre-war pattern of European (and world) agriculture as closely as possible, since this would meet neither the requirements of many regions—the need, for instance, of some of the poorest agricultural regions for a more intensive system of production based on the demand of an industrialised neighbourhood—nor changes in consumers' demand the trend of which before the war was in favour of non-cereal foodstuffs.

It is, moreover, almost certain that European agriculture will have to reckon with a permanent loss of population, since many of those who left the country during the war will not return. This happened after the last war, and there is every likelihood that it will happen on an even larger scale this time.

In addition to its immediate and practical interest, the book should prove indispensible to the future economic historian who will undertake to study the European food situation in the two world wars on a comparative basis.

CHARLOTTE LEUBUSCHER.

The Future of Colonial Peoples. By LORD HAILEY. Issued under the auspices of the Royal Institute of International Affairs. Oxford University Press. 1943. 63 pp. 3s.

This book reproduces a course of lectures given by Lord Hailey at Princeton University in February, 1943. The issues were therefore obviously chosen and represented with a view to answering certain criticisms and demands put forward in American public discussion during the last few years. It would be erroneous, however, to see in the book mainly an apologia of European and, in particular, British colonial policy. On the contrary, the study is remarkable for the objectivity with which controversial questions are discussed and considered from the point of view of the various interests concerned.

One of the principal facts which are impressively brought out is the great diversity of colonial problems—beginning with the different ways in which colonies were established, and evident to-day in the very different stages which they present with regard to economic development, social structure and political maturity. Hence the futility of generalisations on colonies and the way in which they should be governed. Few of those, for instance, who demand that "the" colonies should be liberated from external control, are aware that one way of bringing this about is the absorption of the colonial territory, with full political rights, into the body politic of the metropolitan power—the method applied by France to Algeria, for instance.

The political future of colonial peoples is Lord Hailey's central theme, while other aspects, such as economic and financial policy, are more summarily dealt with. It throws a significant light, both on the point which public thought on colonies has reached, and on Lord Hailey's own position, that the question which emerges as major political issue is how colonial peoples can best be guided towards self-government without imperilling the system of law or the social and economic services which European administration has provided. There can be no doubt that considerations of this kind could be ignored only at great loss to the colonial peoples themselves, and it is impossible to question Lord Hailey's assertion that "it would be an abnegation of the democratic ideal to suggest that Northern Nigeria would benefit by being restored to the uncontrolled rule of the emirs, or Malaya to that of its sultans". Nor would it be desirable if the small educated class of natives, in West Africa and elsewhere, were submerged by the more numerous primitive elements, or if self-government should work out as "the unregulated domination of small European minorities over indigenous peoples".

Another important point which is emphasised is the close connection between the evolution of self-governing institutions and a dynamic programme of social and economic development. In other words, a higher standard of living and a more diversified social structure are not only in themselves desirable objectives, but "must be viewed as an essential preparation for the enjoyment of self-government".

The provision made for assisting the colonial peoples to attain maturity for self-government is also used as the main criterion in a review of the policies followed at present by the various controlling powers. An extraordinary amount of interesting information is compressed in the section—less than twenty pages—devoted to this subject. Another Part deals, more briefly, with various proposals for some form of international control of colonial administration, leading up to the recommendation to set up Regional Councils, a scheme which Lord Hailey has developed in greater detail on previous occasions, and which may be regarded as the accepted policy of the British Government.

Altogether, the study should render valuable service towards a better understanding of colonial issues by an interested public in many countries, and it will rank in the future as an important statement of principles made at a decisive juncture in the development of colonial affairs.

CHARLOTTE LEUBUSCHER.

The Population Problem in India: A Regional Approach. By N. V. SOVANI. Gokhale Institute of Politics and Economics, Poona. Publication No. 8. 1942. 221 pp. Rs. 4 or 8s.

The astounding growth of population in India revealed by the Census of 1941 aroused widespread public interest in a problem which has long been recognised by economists as fundamental. How can the (miserably low) standard of life of the Indian masses ever be raised substantially, so long as any improvement in resources or productive capacity is immediately swallowed up by an increase in the population dependent upon those resources?

No thorough statistical analysis of the growth of population in India has hitherto been attempted, partly because of the inadequacy and unreliability of the necessary data (in particular of the vital statistics), and partly because of the mere size of the problem. India has rightly been compared with a Continent (or sub-Continent) rather than with a "country", and the great heterogeneity of conditions and resources (including the make-up and density of population) necessarily means that the total trend of population growth disguises many contradictory and conflicting trends appertaining to sub-divisions of the "country" and of its peoples, and hence cannot permit of any generalised interpretation. At the instigation of Mr. D. R. Gadgil, Director of the Gokhale Institute, Mr. Savani has attempted to approach the subject more realistically by selecting a series of more or less "typical" and homogeneous (or integrated) regions—based on Census divisions providing the necessary separate statistics—for each of which he has collected all the important available and relevant facts on economic conditions and on the composition and movement of population, with the object of searching in each case for any well-defined trends in population movement and any significant correlations between physical environment, economic circumstances and population composition and growth. This theoretical approach is sound, whilst the statistical devices employed are appropriate and well designed to make the best use of the limited, and in certain respects defective, material at his disposal. The statistical computations are kept in the background and not permitted to interrupt the layman's reading of the text.

This approach is far more hopeful than any attempt at treating population growth in India as an undivided whole, and although Mr. Sovani's investigation is admittedly only a "sample inquiry", aiming at analysis rather than prescription, the results are sufficiently fruitful (although far from complete or final) to justify the choice of method and to give hope that further work will be done along similar lines in the future.

Mr. Sovani has selected 22 regions, scattered throughout India, and chosen so that taken together they " constitute a fairly representative sample of India ". It is, therefore, a selected, not a random sample. For each region he then sets forth the relevant facts and figures—economic and demographic. These refer mainly to the period 1881—1931, but where possible figures were added for the 1941 Census (with which Appendix III deals in more detail). Next, these facts and figures are discussed and as far as possible interpreted. Finally, he sums up his tentative conclusions.

He discovers that these 22 regions fall into four groups: (1) Very-rapidly-growing-regions (with a recorded population growth of not less than 60 per cent. between 1881 and 1931): seven regions fall in this group; (2) Fast-growing-regions (20–45 per cent.): four regions; (3) Very-slowly-growing regions (10–16 per cent.): five regions; (4) Stagnant-and-declining-regions (less than 10 per cent. increase and in some cases an actual decrease): seven regions.

A number of significant correlations between economic factors and population growth emerge from this analysis, which cannot be summarised here. For India as a whole it is concluded that the birth-rate shows no significant movement since 1900, and hence that the death-rate remains the decisive factor controlling growth of population. Finally, Dr. Gyan Chand's conclusion is quoted with approval that in India " the reconstruction required cannot be carried out without making the control of population an integral part of the whole scheme ".

VERA ANSTEY.

Modern Commercial Policy: A Study in Technique. By R. D. TIWARI. New Book Company, Bombay. 1942. 473 pp.

Mr. Tiwari has attempted to include in one volume a range of subjects sufficient for some half-dozen full length works. He outlines the commercial policy of the principal Great Powers since the end of the 18th century, dealing in some detail with developments since the Great Depression; he analyses the nature and content of commercial treaties; discusses exchange control, the quota system, the Ottawa Agreements, the trade agreements programme of the U.S.A. and of Britain, and Indian commercial policy; and finally, makes recommendations for the future. His review ends at 1939, but his recommendations relate to the post-war world. It is hardly surprising that his treatment is, in some respects, superficial, and that he fails to provide adequate supporting evidence for some of his conclusions. On the other hand, he has managed to summarise concisely, and on the whole fairly, an immense amount of descriptive material, and explains clearly and systematically the various types of mechanism employed by the chief commercial countries to implement the commercial policies pursued. In general it can be said that the descriptive and explanatory parts of his work (and these form the bulk of the book) afford a useful introduction to the whole subject and one which cannot be found elsewhere in such short and convenient compass. His summaries of the nature and content of the principal trade treaties of the U.S.A., U.K. and India are particularly useful.

The work is less satisfactory when an attempt is made to pass from exposition to an evaluation of the results of the policies pursued. For instance, after an admirable account of the mechanisms of exchange control he concludes that " some form of moderate exchange control will have to be maintained even after currency stabilisation is achieved ". What does this imply? No further explanation is given and one wonders what is meant by " moderate exchange control ". In his chapter on the Ottawa Agreements his statistics are confined to values, used without reference to price changes, and these alone do not suffice to substantiate his criticisms and conclusions. Even if one agrees in the main with his general condemnation of the Ottawa policy, it cannot be denied that he exaggerates British gains and belittles the benefits to the Dominions (and India). In other words, his conclusions go far beyond what is warranted by the evidence adduced.

Mr. Tiwari's general attitude is cautious and orthodox. He believes in the benefits to be derived from an expansion of international trade, the abolition of discrimination, and a reduction in trade barriers, except in so far as the latter can be justified by the Infant Industry argument. His bugbear is Imperial Preference.

VERA ANSTEY.

A History of the Liberal Party. By the Rt. Hon. Sir HENRY SLESSER. P.C. Hutchinson & Co. (Publishers), Ltd. 1944. 172 pp. 12s. 6d. net.

A history of English Liberalism, as a history of ideas and their influence on politics, would be one of the grandest themes a historian could choose. A history of the Liberal Party is not quite the same thing. Yet it could still be made a story as instructive as exciting. The present book hardly does so. Overcompression has turned the greater part of the story into a dry outline of the main facts, and lack of sympathy with Whiggism and mid-nineteenth century Liberalism on the part of the author do not help to make the subject more attractive. One indeed sometimes wonders what has made Sir Henry Slesser undertake so uncongenial a task. It is only when he comes to the later Radicalism of Joseph Chamberlain and Dilke that the story assumes life. And it is mainly in the very last sections, when the author speaks of the events of his own lifetime, that the book becomes really interesting and valuable.

This is not to suggest, however, that the book suffers from any strong political bias. Indeed, if one did not know that the author has been Solicitor-General in the Labour Government of

1924–1929 one would hardly guess his political affiliation till he himself explains them towards the end of the book. While he does not conceal his own views, no coherent picture of them emerges and his occasional and sometimes very shrewd comments leave a somewhat puzzling general impression. In the 1870's it was “ the more enlightened Tories ” under whose auspices “ the great change from Individualism to collective actions ” took place. Then it is Joseph Chamberlain who takes the centre of the stage and it is interesting to see the author argue that “ it is at least possible, had he remained to lead the Radicals, that the Labour Party, which ultimately undermined the confidence of the working people in Liberalism, would never have come into being ” (p. 125). Yet at the same time the author cannot help regretting the disappearance of that “ individualistic pacific outlook of which Gladstone, Harcourt and Morley were surviving ~~representatives ” and~~ its replacement by “ the new conception of the Kingdom and Empire as an estate, new, that is, since the Industrial Revolution, [which] had shown itself in the disappearance of objections to the use of the State in internal betterment ; but this was only half the story—if the State was once more regarded as an entity in the domestic sphere, so also, figuring as the Nation it had come to claim a new vigour in Foreign Affairs. . . . The Nation, as it is said, had ‘ found itself ’ ; whether in the process the individual has not been lost is still to be discovered ”. And the author duly points out that it was one of those “ advanced Radicals ” who were among the first to speak of the “ grandeur of our Race, already girding the earth, which it is destined, perhaps eventually, to overspread ” (p. 134). He cannot help, in Foreign Affairs, preferring the “ Cobdenite conception of nations and their peoples as trading entities peacefully exchanging goods and services for the benefit of the whole world—a ‘ bagman's millenium ’, as that early Fascist, Carlyle, called it ” (p. 143) to the Imperialism of the Radicals. Still later his sympathies are clearly with Campbell-Bannerman and against the “ little group of highly intellectual gentlemen, Grey, Haldane and Asquith at their head ”, and still more against the Conservatives, who allowed some legislation to pass “ because, as a matter of party tactics, they deemed it expedient not to quarrel with organised labour. Their behaviour,” he continues, “ was quite without principle ; if there ever was a measure which might reasonably have been rejected, it was the Trade Disputes Act, which gave a complete immunity to Trade Unions for all wrongs committed by them, not even limited to those done during a Trade Dispute : it was not even a Government Bill, yet the Conservatives let it pass ” (p. 148). When he comes to Lloyd George's National Health and Unemployment Insurance Act the author's apprehensions become even more marked : “ It contains feature which have left a lasting mark upon the policy of this country ; in the first place, in order to carry out its provisions it gave to the Minister great powers of a novel order, to legislate by order and regulation, under the Statute, without further consultation with Parliament—a power found so attractive to the Bureaucracy and so speedy in its operation that nearly every Government Department exercising administrative functions is now endowed with similar authority. In the end it may operate to displace parliamentary control of the executive. It was not content with administrative regulations ; the Bill also in effect excluded the Courts from nearly all jurisdiction to decide whether a particular person was or was not entitled as a matter of law to receive benefit.” And we find even the statement that “ the uncritical acceptance of the *Beveridge proposals* (themselves containing potential limitations of freedom) by modern Liberals must, to an old-fashioned individualist, prove disquieting, nor is there any recognition of the growing danger of central control of local authority by nationally appointed regional officers or otherwise.”

The book could have been much improved by a more careful revision. There are a number of irritating minor slips, such as that on p. 134 when Balfour is made Prime Minister (apparently in 1895) in the paragraph before it is explained that in the Cabinet in question he became in fact First Lord. And the statement that “ the pound of 1900 was now [1913] worth only sixteen shillings ” is apparently intended to mean the opposite from what it says.

F. A. v. HAYEK.

La tradition littéraire des idéologues. By EMILE CAILLET. With an introduction by GILBERT CHINARD. (Memoirs of the American Philosophical Society, Vol. XIX.) The American Philosophical Society, Philadelphia. 1943. xxii + 322 pp. $4.00.

Though in no way directly concerned with economics, this book ought some day to provide the background for one of the more interesting chapters in the history of economics which has yet to be written. The *idéologues* are to-day almost forgotten and their name persists only in the perverted meaning given to it by Napoleon and Marx. But among their claims to greater attention than they have received their contribution to economics is not the least important. From their master Condillac they took over the subjective approach to the value problem. J. B. Say was long an active member of their circle and Destatt de Tracy probably the most successful continuer of Condillac's utility analysis till Louis Say carried it still further.

Till recently M. F. Picavet's bulky and not too readable study of the group, published in 1891, was the first and only coherent account of their doctrines. Nine years ago Professor C. H. Van Duzer in *The Contribution of the Idéologues to French Revolutionary Thought* (Baltimore, 1935) gave us an English account of their work, and a little earlier Professor G. Chinard elucidated in several studies various aspects of their activities and particularly their relations to Thomas Jefferson. But Professor Caillet's new work is the first which clearly exhibits the precise position of their work in the secular tradition of philosophic thought. There is, perhaps, even too much discussion of the background and the later continuation of their work in this new study compared with what we learn about their own work. But this is partly explainable by the programmatic character of much of the work and the premature end of their activities. Professor Caillet is perhaps also too much inclined to claim them as predecessors of Comte and his brand of positivism, which probably derives more from Cuvier and the physicists than from their "essentially moderate" position which the book rightly stresses. But with the author's evident sympathy for the Comtean tradition he hardly has an alternative: either one must regard the *ideologues* as men who had too much good sense to pursue to extremes some of the more rash pronouncements about the treatment of the study of ideas as a branch of zoology of which they were occasionally guilty, or one has to see them as an earlier attempt of a movement which culminated with Comte and produced its last great figure in H. Taine.

It would be inappropriate to pursue these questions of the history of ideas any further in this place and we must confine ourselves to recommending the work as one of the best type of French studies of their kind—with the added advantage of the lavish production which to-day one scarcely meets outside the publication of American learned institutions. Its usefulness is further increased by a full bibliography.

F. A. v. Hayek.

The Decline of Liberalism as an Ideology. With Particular Reference to German Politico-Legal Thought. By John H. Hallowell. University of California Publications in Political Science. Vol. I, No. 1. University of California Press, Berkeley and Los Angeles. 1943. xiv + 146 pp.

Few studies in the history of ideas could be more interesting and instructive than a really good account of the decline of liberalism in Germany—a decline which started before it had taken roots outside the realm of theory and closely connected with the fact that to Germany liberalism came not before but simultaneously with nationalism and socialism. To produce such a study would be a task of very considerable magnitude which no one without a very intimate knowledge of German history and German ideas ought to undertake. It is not evident that the author of the brief study before us possesses many of the required qualifications—hardly to be expected in a doctoral dissertation. He confines himself largely to the legal aspects of his problem, the elaboration and transformation of the concept of the *Rechtsstaat*, and there is no reason to quarrel with him on that account; it is a subject quite big enough for a valuable monograph by an author thoroughly familiar with it and with a knowledge not limited to this particular field. But though our present author sees some of the real problems, there is more evidence of dependence on a few secondary authorities, such as E. Troeltsch, H. Heller, and a few articles in the *Encyclopædia of the Social Sciences* than of a comprehensive study of the original authorities. Even such writers as Fichte or Mazzini are frequently quoted at second hand and it is thus not surprising that e.g., Fichte, because of his early liberal views, is defended against the "unjust" description as an ideological forerunner of the National Socialists (one wonders whether the author has ever read the *Geschlossene Handelsstaat*). The result is, on the whole, not much more than a summary of text-book views which, though they illustrate important tendencies, makes them little more intelligible than they were before.

As on the historical so on the conceptual side there are many signs that the author has tackled important problems and with a sound instinct, yet with quite inadequate means and before he has gained sufficient clarity on what precisely he means by the terms he employs. This is well illustrated by two of the central problems of the study, the influence of positivism and the effect of the formalisation of the law. That "the decline of liberalism parallels the degree to which liberal thinkers have accepted positivism" is a just and significant if not original observation. But the elaboration of the argument is vitiated by the ambiguity of the term "positivism" as it is used, which appears to cover a variety of different and not ncessarily connected intellectual attitudes. With regard to the concept of "formal" law the ambiguity is even more serious; the term appears to be used to describe two aspects of law which are certainly different and perhaps sometimes even contradictory; on the one hand the fact that a rule has been imposed by the proper constitutional machinery, and on the other that it is a true general rule, meant to apply to yet unknown people in circumstances which cannot be foreseen in detail and in this respect different from any legislative measure designed to achieve a specific end.

The ambitious purpose outlined in the Preface, "to ascertain *when* and *how* liberalism as an ideology became decadent" can scarcely be said to have been achieved by this study. It may, however, serve to direct attention to certain limited aspects of a big problem which certainly deserve examination, but on which a great deal of detailed work unknown to the author has already been done, and a great deal more will yet have to be done, before a comprehensive study like the present can be attempted with better prospect of success.

F. A. v. Hayek.

The Spirit of American Economics. By J. F. Normano, with a supplement: *The Development of Canadian Economic Ideas*, by A. R. M. Lower. A publication of the Committee on the Study of Economic Thought, distributed by The John Day Company. New York. 1943. 252 pp.

The expectations aroused by a volume advertised as the first of a new series of *Studies in the History of Economic Thought*, sponsored by a committee consisting of European and American scholars and supported by the Latin American Institute, are only imperfectly fulfilled by the contents. The author, described on the jacket as a doctor of the Universities of Petrograd and Freiburg im Breisgau, and a former member of staff of the Universities of Petrograd and Harvard, wields a versatile pen. But his intention, announced in the Preface, of writing history as though it were literature, has not produced a very satisfactory result. The interesting subject is treated in a slight and somewhat journalistic manner, and the attempts to enliven the exposition by numerous quotations in foreign languages are more irritating then pleasing. Dr. Normanot is very anxious to discover a specific American spirit in the economic literature of that country, but though the opening chapter leads the reader to expect something very different from the traditional view, the author can scarcely conceal his own disappointment when nothing more striking than the familiar figures of H. C. Carey, Henry George and E. Bellamy emerges. The author's lack of sympathy for economic theory hardly allows him to do justice to the achievement of economists in that field, nor, one suspects, has it assisted his familiarity with their work. One can hardly expect much information about what to an outsider seems most interesting in American economics from an author who has to say little more about F. W. Taussig than that after the last war he " continued to teach as he had in the seventies, eighties, and nineties of the last century "

In general, though there is some interesting information about certain early American economists in the volume, which will be new to most European economists, for the last eighty years even the treatment of the figures whom the author thinks significant is so sketchy as to be of little help. This may be partly due to the fact that the author does not seem quite to have made up his mind whether he wished to write a history of economics or a history of " economic ideas " in the sense of ideals governing current economic policy. While he inclines on the whole to the latter, there is little serious attempt to provide the political and economic background against which alone such a history of ideas on economic policy could be made enlightening. Generalities such as that towards the end of the 19th century " the official political economy, representing capitalist interests, was English-Austrian; the socialist protest against this political economy was imported from Germany and directed by immigrants " are not very helpful.

The Supplement on *The Development of Canadian Economic Ideas*, by Professor A. R. M. Lower, of the University of Manitoba, gives a brief sketch of the economic history of Canada with a few comments on the main issues of economic policy and the outstanding figures in their public discussion. As the author refrains from discussing the work of living economists and " the first Canadian book on theoretical economics has appeared in May, 1942," we learn practically nothing about the state of economic science in the Dominion beyond the bald statement that " there is now a respectable body of Canadian economic thought which in breadth and learning goes far beyond the pamphleteering of former days ".

F. A. v. Hayek.

Statistics. By L. H. C. Tippett. Home University Library. Oxford University Press, London. 1943. 184 pp. 3s.

Statistical Analysis and Biology. By K. Mather. Methuen & Co., Ltd., London. 1943. 247 pp.

The two books under review are designed for different purposes and will appeal to different classes of readers. Mr. Tippett has given the answer to the teacher's prayer for a non-technical, introductory book on Statistics for students with a very limited mathematical knowledge. Within the short compass of 184 pages he succeeds in presenting very simply and concisely the greater part of the problems with which modern statistics is concerned. His emphasis is mainly on the use of statistics in economic and social problems though he finds time to deal with applications to quality control and scientific experimentation. The emphasis throughout is on the underlying ideas rather than on techniques. Sampling, correlation, forecasting, and market surveys are some of the topics discussed. This admirably balanced book lives up to the high standard that we have come to expect from the Home University Library and should be put into the hands of every student of Economics.

Dr. Mather's book has an entirely different aim. It seeks to explain the application of statistical method to biological and agricultural problems, and its approach, based mainly on the analysis of variance, is unfamiliar to those who are used to economic statistics. That it should be unfamiliar is to be deplored. Statisticians working in the social field have much to learn from their colleagues who work in the natural sciences, but at present a great part of the latters' work is unintelligible to them. The theory of experimental design and the analysis of variance should, however, prove as useful a tool in economic and social investigations as it has in agriculture and biology. Unfortunately Dr. Mather's book will not fill the gap. Parts of it are very obscure and the book as a whole is marred by misprints. Moreover, in his anxiety to avoid difficult mathematical argument, most of the proofs on which the methods are based are omitted. Yet the reader is expected to have a fair knowledge of mathematics. For instance, on p. 43 the symbols $\frac{N-1}{1}!$ and $\frac{N-2}{2}!$ are used without explanation, and the whole work is full of cumbersome arithmetical formulæ, many of which could have been simplified if more use had been made of the traditional notation. In a book addressed primarily to biologists most of the examples chosen to illustrate the argument are, of course, taken from the biological field and ignorance of the jargon adds to the non-biological reader's difficulties. It is a pity to find these blemishes in a work in which the method of approach is interesting, but the book will be of little use to students in the social sciences who wish to be informed about developments in other fields.

E. GREBENIK.

The Future Population of Europe and the Soviet Union. Population Projections 1940—1970. By FRANK W. NOTESTEIN and others. League of Nations, Geneva. 1944. 315 pp. Paper, 12s. 6d.; Cloth, 15s.

It is strange to think that not so very long ago the main concern of English demographers was with the problem of overpopulation. Professor Cannan first sounded the warning in 1895 that the population of Great Britain would soon cease increasing, but his article was not generally taken notice of. It was in the 'thirties that the general public first became aware of the problem of inadequate reproductivity which confronts us to-day. This was largely the result of the publicity given to Dr. Enid Charles's projections. These were quoted as prophecies, which the author never intended them to be. But the prospect of population decline became a familiar one, and is, on the whole, regarded with equanimity. The present volume, which has been prepared for the League of Nations by Dr. Notestein and his colleagues of the Office of Population Research at Princeton, shows the implication of present fertility and mortality rates on population prospects in different European countries, during the next 30 years.

In the past, the highest fertility and mortality rates declined fastest, and the authors have assumed that this will continue to be so in the future. They have estimated future values of national vital rates by extrapolation on the basis of European experience as a whole. Their results agree with those of previous workers. The projections show a decline in the population of Northern and Western Europe, whilst the population of Southern and Eastern Europe and above all that of the Soviet Union show an increase. The projections for the population as a whole are supplemented by projections of the working population, of the female population of reproductive ages and of the population of dependants.

The authors are very careful in using the term "projection" rather than the term "prediction". But it is inevitable that some of their figures will be regarded as predictions, and it may, therefore, not be irrelevant to speculate on the likelihood of their projections being realised. The great unknown factor is the effect of the present war on long-term trends in vital rates. Dr. Notestein and his colleagues argue that as the last war had no lasting effect on these rates, it is not to be expected that the present one will do so. If this argument be granted, the projections are likely to be correct within a small margin of error. But it is arguable that the social upheavals associated with the present conflict are likely to alter the course of vital rates more permanently than did the last war. In this country and in Germany predictions regarding the wartime course both of fertility and of mortality have not been fulfilled and wartime experience does not give any very clear pointer to the future. But even if Dr. Notestein and his associates should prove mistaken in their belief in the stability of vital rates, they have performed a valuable service in preparing projections of population which may be compared for different countries of Europe. Given the imperfections of their basic material (the vital statistics of some European states are far from reliable) they have performed the task they have set themselves with great skill. In the past projections or predictions of population have not been conspicuously successful. It is no disparagement to the present authors to express a hope that their rather gloomy projections for Northern and Western Europe will prove wrong.

E. GREBENIK.

La Revue Economique et Sociale. Revue Mensuelle d'Etudes et d'Information Economiques. Alger, Paris. No. 1, 71 pp.; No. 3, 108 pp. 25fr. or 3s. per number. Subscription for 12 numbers 250 fr.

The first third, and fourth number of this new monthly journal have just been received. All are without a date and give, on the covers, Algiers and Paris as place of publication, though the actual address given is 14, rue Michelet, Alger. It appears to be edited by M. J. P. Raudnitz, who contributes to the two earlier issues, which contain, among others, articles by A. B. Lavergne and G. H. Bousquet. The fourth issue opens with a translation of Sir William Beveridge's Preface to Mr. Charles Madge's, *Industry after the War*. Though the journal appears to be designed mainly for the discussion of problems of reconstruction and for the exchange of information with the Allies, articles on more general topics such as the economic functions of the State, the future of liberalism, the policy towards monopolies and the relations between technology and the spirit of capitalism fill the greater part of these early issues. The substantial section headed "Documentation" at the end of each issue consists largely of reports about English and American official documents and periodical articles.

The ambitious attempt to start at once with a monthly journal, devoted solely to economic problems, testifies at the same time of admirable courage and determination, and of better facilities and a more numerous personnel than one would have expected to find at Algiers at the present time. It is to be hoped that the eloquent appeal for support from contributors, readers, and subscribers, with which the first number opens, will find all the response it deserves.

F. A. v. HAYEK.

SHORTER NOTICES

The Madras General Sales Tax Act. By B. V. NARAYANASWAMY NAIDU and S. THIRUVENGADATHAN. Anamalai University, India. 1940. 217 pp.

This pretentious volume seems to have been inspired by the desire to defend the Madras Sales Tax. It has no theoretical background and covers only the first year of the tax. It is, therefore, hardly surprising that the authors have been unable to secure evidence that enables conclusions to be drawn as to the effects or value of the Act.

Relief Deliveries and Relief Loans, 1919-23. League of Nations. Allen & Unwin. 1943. 62 pp. 3s.

This work presents in clear concise form a picture of what was done. The statistical tables show what commodities were provided, at what cost, by what countries, to be consumed where, and to meet what needs. The organisations employed are indicated, and the whole is accompanied by some pertinent comment. In particular it is pointed out that the consumption of redundant stocks of any product may relieve markets as well as want, and on the other hand that a country suffering privation may be quite capable of repaying the cost of relief later on. Nevertheless the arrangement of a lending operation may involve political complications causing disastrous delays (pp. 29–31). "The sale of relief commodities for cash, if pushed too far, may involve the risk of unduly depleting reserves [of foreign exchange] and of entailing currency depreciation." Germany was made to pay $282 millions in 1919. But it may perhaps be commented that Germany was in any case being made bankrupt and whatever assets were used for subsistence were released at the expense of her creditors.

Social Studies and World Citizenship. A Sociological Approach to Education. By L. J. F. BRIMBLE and F. J. MAY. Macmillan & Co., Ltd. 1943. xii + 158 pp. 6s.

This book has little to do with "social studies" as readers of ECONOMICA will understand the term. Even the sub-title is misleading unless "sociological" is taken not in its scientific meaning but in the popular sense of awareness of social problems. The purpose of the somewhat disjointed chapters it contains is evidently to advise the school-teacher of how to bring out more strongly the social implications of the various subjects he has to teach. It seems a little doubtful, however, whether the scraps of information here collected or the personal opinions expressed can be of much use to anybody. What, e.g., do they expect a child to write on the recommended essay subject of "The Sociological Advantages of Travel"? One is left wondering whether the authors themselves know what they mean by "sociological".

Railway Traffic Expansion and Use of Resources in World War II. By T. HULTGREN.

National Product : War and Pre-War. By SIMON KUZNETS.

Production of Industrial Materials in World Wars I and II. By GEOFFREY H. MOORE.

(Occasional Papers No. 15, 17 and 18 of the National Bureau of Economic Research.)

These pamphlets in the "Our Economy at War" series deal with some aspects of the adaptation of the American Economy to a war-time footing. Dr. Kuznets studies the American national product and gives reasons for differing from the official estimates made by the Department of Commerce. Mr. Moore's study supplements that of Dr. Kuznets. It deals more fully with the production of industrial materials, and compares the experiences of this war with those of the last. Mr. Hultgren explains how the American railways met and overcame the problem of traffic expansion, which our own railways encountered at the beginning of the war and illustrates his description by means of some interesting statistics.

A Re-Calculation of the Poverty Datum Line. By Professor EDWARD BATSON. Series of Reports and Studies issued by the Social Survey of Cape Town. (Revised Edition of Report No. SP. 3.) Report No. RS. 203. May, 1944.

In this short report the method of construction of the poverty line employed in the Social Survey of Cape Town is briefly described. The effects of the war-time rise in prices on the poverty line are also calculated.

The Tennessee Valley Authority : A Study in Public Administration. By C. H. PRITCHETT. The University of North Carolina Press. Chapel Hill. 1943. xiv + 333 pp. $3.50.

This, as the sub-title indicates, is a study primarily of public administration and therefore does not ask quite the questions to which the economist will be most interested to know the answers. But for anyone who wishes to make a serious study of this interesting experiment, the book will provide an indispensible companion that provides clear and compact information on some important aspects on which it would be difficult to find it elsewhere. The chapters which the economist will probably find most interesting are the fifth on "Regional Planning and Development" and the last on "Personnel Administration". Though, like so many writers on this subject, the author is apt occasionally to be carried away by his enthusiasm for his subject, the study is, on the whole, more detached and balanced than one has come to expect in most things written on the subject.

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The Agreements of Bretton Woods

By Ernest H. Stern

The Currency Agreements of Bretton Woods are detailed regulations of the manner in which the 45 United Nations intend to deal with each other in respect of payments and payment obligations to each other and in respect of the value of money used for payments to each other. They require comment:—

firstly, on the principles underlying the whole scheme, their soundness and, indeed, the workability of the scheme;

secondly, on the significance that attaches to some of the detailed regulations, as e.g., to the regulations regarding international money transfer in general, exchange rates, gold, the gold clause, the distribution of power and the distribution of the share in the amenities and contributions;

thirdly, on the usefulness of various regulations from the point of view of the technique of international payments and of banking.

The following review deals with the first two categories, viz., in Sections I to IV with an examination of the principles applied and the workability of the scheme, and in Sections V and VI commenting on some details of wider significance. The third category has been excluded; it does not appear to be of sufficient importance at present.

I

The Agreements set up a machinery for international co-operation, but limit its scope to co-operation in respect of international payments and the international value of the means of payments. Adherence to the Agreements, even after signing and ratifying them, is a completely voluntary one. A member may withdraw from them at any time with no more ado than giving notice. Co-operation is provided in three ways, as follows:—

(1) The Agreements lay down *Rules of Conduct* in valuing, and in dealing with, members' currencies and gold. The main Rules of Conduct imply freedom from Government restriction in respect of payments in current transactions and no alteration of exchange rates other than through, or in accordance with, the rules set up by the Fund. Gold is used both as a medium of expressing the rates at which the currencies may be exchanged and as an auxiliary means by which to carry through an exchange of different currencies. It has been chosen as an international currency which belongs to no particular country, and which can, and within certain limits may, serve as international legal tender. The Rules of Conduct tell members how they should behave in the field of foreign exchange or gold, if their nationals deal with abroad, but do not impose any obligation on members, or their nationals, to enter into any deals concerning foreign exchange or gold. There is, however, one exception, which is commented upon in Section V on page 175, paragraph (i).

(2) They provide *Banking Facilities* for the settlement of " current payments " by way of exchange of members' currencies, such banking facilities including arrangements for a limited supply, on short-term credit, of members' currencies for the sake of facilitating the settlements. The banking facilities will be provided by the International Monetary Fund, hereafter, for short, called the Fund, which will deal only with members' monetary agencies. The Fund resembles a World Reserve Bank, without the right to issue money.

(3) Lastly, they provide *Long-term Facilities* for supplying foreign exchange for the settlement of a limited range of " non-current " international payments. These long-term facilities are provided by or with the help of the International Bank for Reconstruction and Development, hereafter, for short, called the Bank.

Translated into the language of daily life, the meaning of the Agreements may be expressed as follows:—" The 45 United Nations have resolved, in respect of one particular field of intercourse, in future to behave as reasonable, grown-up persons. They will regard themselves as a community of nations, each member being master in his own house and responsible for his own welfare, but in their dealings with each other, members will observe a code of decent and polite conduct, and will even, as it behoves members of a community, help each other occasionally in a good-neighbourly fashion. They have, at the same time, resolved in respect of this one field of intercourse, in future to resist the childish impulse of asserting themselves and their supposed rights at every instance, and no longer to clash in open conflict when dealing or competing with each other ". The members do not relinquish any of their sovereign rights, but they have resolved to exercise them as grown-up nations, i.e., with restraint in respect of manners as well as of objectives. Each member remains free and responsible to his own national institutions as to how to put his house in order, but in his dealings with his neighbours he has taken the road at the end of which stands the high principle " to do unto others as you would they should do unto you ". Unfortunately, the nations have only entered the road and then stopped at a half-way house. The examination of the scheme will reveal how far they have progressed and how far they have yet to travel towards the gate that leads to peace and security. However, to have entered that road and struck out in the right direction would mark the beginning of a new epoch, provided, of course, the resolve of the Governments' representatives at Bretton Woods is matched by the resolve of their treaty-making bodies.

Let it be noted too that the Agreements are strictly limited to one field of intercourse amongst the United Nations, i.e., that of payments to each other and of valuing the money to be used in international payments. There are many other fields where the same nations will meet in competition and exchange, as, e.g., trade investment, migration, politics. If they were to resort to strife and lawlessness in those other fields, clearly the agreements on currency could do little in furthering peace and prosperity among them. Their high resolve in respect of the regime of their International Payments may thus be completely nullified. Lord Keynes, the Chairman of the British Delegation at Bretton Woods, underlined this fact in two letters to the Editor of *The Times* (August 24th and 30th) in which replying to critics of the Agreements he observed, firstly, that the Bretton Woods Agreements set no limit to the commercial policy of any member, and, secondly, that " forms of commercial policy may be so destructive of multilateral trade that, if they are adopted, Bretton Woods will have been rather a waste of time ".

This possibility of the United Nations failing to act as grown-up members of a community of nations in the various other fields of intercourse cannot be overlooked after the sad experience of recent history. It supplies no ground, however, for spurning an understanding, if agreement can be reached in respect of one particular field. There is wisdom in attempting to solve one problem at a time, and the problem of International Payments is, at this juncture, complex enough to justify restricting to this one object a novel arrangement in which 45 nations, big and small, have to concur. No doubt wisdom will turn into folly if a partial solution is acquiesced in and made to substitute for a final solution. It is obvious, therefore, that Bretton Woods must be looked upon as only a beginning, the first of a series of agreements or equivalent acts of co-operation. Its importance is not only derived from the provisions that attempt to regulate international payments—they will be dead letters if the currency agreements remain an isolated attempt—but also from the fact that they have set an example to co-operation in other fields. They show a way how to apply on a practical object high principles of international intercourse.

A great opportunity was offered to the governments. How much have they risen to it? The examination to follow will reveal this. It will judge the value of the Agreements mainly from two points of view, viz.:—

(a) How far they contribute to balancing the supply and demand for foreign exchange that will arise after this war, and by so doing contribute to creating conditions which make for the stability of their respective values in terms of each other;

(b) How far they assist in introducing into the dealings of nations fairness and respect for other peoples' interests in the place of strife and self-assertion.

II

How far do the Bretton Woods regulations contribute to balancing the supply and demand for foreign exchange? They intend to regulate the market by " canalising " through the agency of the Fund, the payments for what is termed " current transactions ", and by opening, through the agency of the Bank, additional supplies of foreign exchange for a certain number of " non-current transactions." The payments for " current transactions " comprise (XIX, i):—

(1) All payments due in connection with foreign trade, other current business, including services, and normal short-term banking and credit facilities.

(2) Payments due as interest on loans and as net income from other investments.

(3) Payments of moderate amount for amortisation of loans or for depreciation of direct investments.

(4) Moderate remittance for family living expenses.

Moreover, the Fund may determine whether certain specific transactions are to be considered current transactions or capital transactions.

" Current " payments shall not be subject to transfer restrictions, at least not without the approval of the Fund, nor to any discriminatory practices. These provisions are indeed most important, but whether they can be adhered to will depend on the demand for foreign exchange being satisfied. Exceptions are foreseen for the period of transition. The current payments are apparently supposed to be, or at least treated as if they were, equilibrating or self-liquidating, though not within a few months, but within a few years. Balances that may arise are supposed to be of a temporary nature, and out of funds contributed co-operatively by all members, short-term credits are arranged to help any member to settle such balances.[1] The temporary self-liquidating nature of the balances and resulting credits and the comparatively small amounts envisaged are stressed by the provisions :—

(a) That no member may use the resources of the Fund to meet a large and sustained outflow of capital (VI);

(b) That credits granted during 12 months shall not exceed one-quarter of the debtor's " quota " (V, 3, a, iii);

(c) That credits granted shall in no case aggregate to more than the quota (V, 3, a, iii), and lastly,

(d) That the quotas themselves are small in comparison to what used to be the debit side of the foreign exchange balance of the respective members.

A few instances will illustrate the point. Belgium has been allotted a quota equivalent to U.S. $225 Million and, therefore, an annual credit facility with the Fund equivalent to U.S. $56 Million and a maximum aggregate line equivalent to U.S. $225 Million. This compares with imports alone equivalent to about U.S. $850 Million p.a. in 1937/38, and with a substantially greater demand if the other items of current transactions are added and if allowance is made for the increase of the price level and any expansion over the rather contracted size of pre-war current transactions. The four East European members, Poland, Czechoslovakia, Yugoslavia and Greece, taken together, enjoy a total quota equivalent to U.S. $350 Million and, therefore, an annual facility equivalent to about $90 Million and a maximum aggregate line equivalent to $350 Million. Their imports alone amounted to roughly $830 Million p.a. in 1937/38 and their total gross requirements including services, interest, amortisation, with allowance for the increase of the price level and an expansion beyond the restricted trade of the late thirties may easily exceed double that amount. In other words, balances remaining from current payments are supposed not to exceed, say, 5 per cent. of gross foreign exchange requirements in any one 12 months period, and not to aggregate to more than, say, 20 per cent. of such annual requirements over any series of years. Supposing the U.S. were never to require any credit facilities from the Fund, the remaining United Nations could enjoy a total quota equivalent to U.S. $6,100 Million and, therefore, an annual facility of not more than $1,525 Million and a maximum aggregate line equivalent to $6,100 Million. In 1937/38 their imports alone were of the order of close on U.S. $20,000 Million p.a., and their total gross exchange requirements allowing for the increased price level and an expansion of trade were quite possibly considerably more than $30,000 Million, i.e., of an order that the annual line does not exceed 5 per cent. and the maximum aggregate line does not exceed 20 per cent. of one year's requirements, indeed they may fall considerably short of these limits.

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[1] This view was semi-officially stressed by Lord Keynes during the Debate in the House of Lords (May 23rd, 1944) on the Monetary Fund. According to *Hansard* he said :—" I emphasise that such (viz., providing for temporary purposes as payments of imports in periods of cyclical depression) is the purport of the quotas. They are not intended as daily food for us or any other country to live upon during the reconstruction or afterwards . . . The quotas for drawing on the Fund's resources are an iron ration to tide over temporary emergencies of one kind or another ".

The commentator could be more explicit as to the adequacy of the facilities envisaged if the method of calculating the quotas and the considerations determining the limits put to the credit lines had not been cloaked in secrecy. However, the order of magnitude as shown above makes it clear that success of the whole scheme of the Fund depends on two conditions, viz.:—

(a) That current payments of each member actually do eventually equilibrate, and

(b) That the proceeds from current transactions are not diverted to non-current payments.

Using for the sake of illustration the picture of a " canalisation of current payments ", I would say that success depends:—

(a) on the various parts of the canal having been built on a nearly equal level, minor differences being overcome by locks and pumping stations, and,

(b) on locks and other measures effectively shielding the canal from any outer water systems of a different level or on measures to replace any water lost into such outer systems.

This is the crux of the matter. Will conditions prevail in which current payments eventually equilibrate and the proceeds from current transactions will not be required for non current payments? Let us first examine the probability of current payments being self-liquidating. The future cannot be predicted, but the past provides a basis for forming an opinion. For the past it is true indeed that " current payments " in the meaning of the Fund scheme together with " normal business " investments did equilibrate. The fact is well known as regards the 19th and early 20th Century prior to World War I, and need not be demonstrated here. It is also true of the Inter-war period or at least of the 17 years period 1922/38, in contradiction to a widespread belief to the contrary. In a recent article on " Disruptive Factors in International Equilibrium "[1] the writer has arranged the available statistics of International Payments to show that the disequilibrating factors of the Inter-war period were first and foremost the fear-inspired hot money movements, and secondly, the lack of a fair settlement of the costs in foreign exchange of World War I. Note that in both periods market investments of considerable magnitude were forthcoming, but were also necessary for equilibrating the current transactions. The aforementioned two kinds of extra-economic transactions greatly exceeded any balances resulting from current transactions and business investments and but for them the settlement of the business balances would have presented no difficulties. The nations attempted—by hook or by crook—to use the medium of current transactions, particularly trade and services, to provide the means by which to counterbalance those non-current payments, but the latter, especially the hot money transactions, were so enormous and so erratic that the attempt failed. However, current transactions and business investments succeeded to some extent in providing the means for balancing the inequitable distribution of the War Costs among the victor nations of the first World War. Although in the Inter-war period the play of economic forces between nations was greatly interfered with by Government actions, current transactions and business investments exhibited a strong tendency to equilibrate, and their various component factors to adjust themselves to the new conditions. Judging, therefore, from past experience, and assuming Government interference, if any, in the fields of current transactions and business investments being regulated in the manner provided by the Bretton Woods Agreements, it is in fact most probable that the various member nations will be able to balance their current transactions and business investments, though not necessarily in any one particular year. There is, however, an important proviso:—the proceeds from current transactions must not be absorbed by large extra-economic transactions such as hot money movements and payments resulting from an unfair settlement of the costs of World War II. Given such conditions and provisos there is thus good ground to assume that the foreign exchange regime as envisaged by the Monetary Fund part of the Bretton Woods Agreements can succeed. Let it be noted that equilibration of current transactions alone is not required, but that a balancing of current transactions and business investments is sufficient for success, because " current transactions " include interest payments and normal amortisation.

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[1] July, 1944, issue of *The Banker*.

III

I wish to emphasise that provisions against non-current payments absorbing the exchange supplied by, and required for, current transactions are indispensable for the success of the Fund Scheme. Let these provisions now be examined. Judging from the experience of the past there are two main kinds of extraordinary payments to guard against, viz.:—

(a) Hot money movements;

(b) Inequitable distribution of the costs of war, the latter including rehabilitation.

As regards the hot money movements the Agreements impose the obligation on each member menaced by a substantial drainage of this kind to check it. They leave to each member's discretion the manner of prevention (VI, 1 b, ii and 3) This provision will be an effective safeguard as long as members act *bona fide*. Therefore as long as members are co-operating this potential leak may be regarded as stopped, although the Agreements include no detailed regulations.

The same cannot be said of the second category of " non-current " payments, those resulting from War and Rehabilitation Costs. Quite logically, the Agreements say that " the Fund is not intended to provide facilities for relief or reconstruction or to deal with international indebtedness arising out of the war " (XIV, 1). However, quite illogically, they make no adequate provision either for a source from which they are to be paid or for their cancellation. Unless either is done, those non-current payments will drain away the foreign exchange supplied by current transactions. There is no worthwhile source other than current transactions for the supply of foreign exchange for them. Certainly by way of borrowing in the market a debt to a foreign Government may be paid by being turned into debts to private foreigners, a mere change of appearance but not of substance.

Since the beginning of this war foreign exchange for the costs of war have been supplied as follows:—

(a) by some members by way of surrendering foreign exchange and gold reserves and foreign investments;

(b) by some members by way of running up short-term debts, particularly in the form of Sterling funds of foreign countries in London;

(c) by Lend/Lease procedure, the debit balances of which constitute short-term debts;

(d) by outright gifts; of the latter the only instances so far have been those of Canada, but more are now expected to be forthcoming in the form of UNRRA supplies to liberated countries.

The obligations resulting from the supplies of foreign exchange in the form of items (a) to (c), and of the loss of income due to item (a), have altered the distribution of foreign exchange resources among the members so radically and have loaded, as it were, with such unequal weights the scales of balance between the various members that the exchange of services among the United Nations after the war would have to start in a condition of *fundamental* disequilibrium. The funding of the debts would, because of the interest and amortisation included in current transactions, still leave a severe disequilibrium. The war has not ended yet and the debit balances are increasing every day. *For a redistribution of these weights so as to make the exchange begin in balanced conditions, the Bretton Woods Agreements make no* *provisions whatsoever.* Moreover, there will soon be more requirements for foreign exchange, viz.:—

(e) for Rehabilitation, the latter including the repair of war damage, the refilling of exhausted stocks and even the supply of civilian goods during the period of reconverting to civilian supply and exports, productive resources that had been used for war purposes.

It is only category (e) for which the Bretton Woods Agreements make provisions. They do so through the agency of the Bank, which for this reason is an indispensable part of the monetary scheme.[1] However, those provisions are inadequate. They are inadequate not only because of the limitation to this one category—though this would be inadequacy enough—but because they fail even in this one respect to prevent an inequitable draining for the purpose of non-current payments of exchange derived from current transactions. The technical ingenuities embodied in the Bank Scheme, much as they are to be appreciated, cannot balance these weaknesses of principle, the lack of equilibrium with which the international exchange of members is to start. The Bank sets up a machinery for "commercialising" the costs of Rehabilitation, i.e., for assisting the war-scarred members to borrow from abroad the foreign exchange with which to pay for the raw materials, equipment and indirect requirements due to war devastation, exhaustion and reconversion to civilian supply and exports. On such loans interest and amortisation has to be paid, which will form part of the "current transactions". The merit of the Bank exists in setting up a machinery which will greatly assist the war-scarred member nations in *starting* work on reconstruction immediately after liberation and in providing for a "commercialisation" that would be effected *at lower cost* than would probably have to be paid without the help of the Bank. By organising a co-operative state insurance of credits the Bank increases the efficiency of the international capital markets and thus reduces the costs of lending capital, most probably to the benefit of all. However, the scheme has the potential demerit of loading the scales of balance against the war-scarred members by way of burdening them with the interest and amortisation on the commercialised debt. The measure of the actual demerit, overlooking any psychological and political and therewith indirectly economic, reaction, is given by the order of magnitudes involved. When measuring the magnitudes the burden resulting from supplies in the form of categories (a) to (c) above, which has been left unsettled, has to be included. Statistics being scarce at present the commentator can do no more at this juncture than illustrate his point by statistics relating to the U.K. By the end of the fiscal year 1944 the U.K. will have parted with £1,500 Million equivalents of foreign exchange (foreign exchange, gold, foreign investments) and run up short-term debts to the amount of say, £2,700 Million. To this may be added something like £2,000 Million in Lend/Lease account and possibly further amounts, say £300 Million, for post-war reconstruction and reconversion. Assume the costs of these expenditures to be funded, i.e., turned into long-term debt to individuals abroad through the agency of the Bank or otherwise. Assume too the funding to be most "generous", i.e., in the form of cancelling altogether any Lend/Lease balance and charging on the remainder of say £3,000 Million only 3 per cent. on interest account, including fees to the Bank, which are not less than 1 per cent. p.a. and amortising the debt within 25 years.[2] Assume also the assets in category (a) in the past to have supplied an average income of 2 per cent p.a. and that over the next 25 years an equivalent amount of assets will have to be accumulated. In these conditions the "current" payments of the U.K. for the 25 years following the war would be burdened with £250 Million p.a. A period of 25 years has been chosen because political payments of this kind among Allies should not outlast the present generation. However, a distribution over 33 years or even 50 years would not decisively ease the burden of payment. Spread over 33 years the burden would be £208 Million p.a. and over 50 years £165 Million p.a. In the inter-war period the U.K. had an average credit balance of foreign exchange of £152 Million p.a. in current transactions from 1920 to 1924 and £68 Million p.a. from 1925 to 1929 but an average *debit* balance of £27 Millions p.a. from 1930 to 1934, and of £29 Million p.a. from 1935 to 1938. The balances disclose a continuous deterioration of the foreign exchange supply position. If all political payments (payments on Inter-Allied war debt and Reparation Receipts) and charities were excluded, i.e., if current transactions were strictly limited to business obligations, the annual balances would have been better on average by hardly more than £8 Million p.a. One need only add to these pre-war balances the debit balances resulting from this war, of £250 Million p.a. over 25 years and £165 Million p.a. over 50 years in order to realise the tremendous and fundamental disequilibrium with which the Bretton Woods scheme would have to start, if war and rehabilitation costs were "commercialised"—however generously—through the agency of the Bank or similar supplementary institutions. The burden may be made easier to bear by deferring amortisation, say in correspondence to the potential increase of the debtor's national income or rather his capacity to pay foreign exchange. Even without any amortisation the U.K. alone would be faced with the need for adjustment so as to provide indefinitely an additional net balance of more than £100 Million p.a., excluding any Lend/Lease payments.

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[1] Indispensable as it is, no outline of the Bank Scheme was given in the White Paper presented to Parliament immediately before the departure of the British Delegation to Bretton Woods (Cmd. 6519, 1944). Not even the intention to include the scheme was mentioned although the details must have been as well known to the Government as those of the Fund.

[2] A rate as high as 4 per cent. has apparently been envisaged by the delegates themselves. The official Spokesman at the Bretton Woods Conference is reported (*Financial Times*, July 14th, 1944) to have declared at a Press Conference "the rate of interest on loans guaranteed by the Bank would probably be fixed at 4 per cent., with the World Bank getting 1 per cent. as a service charge and the investor 3 per cent.".

The task of this one country in changing her economic intercourse with foreign countries to the extent of improving her balance by more than £200 Millions p.a. over 25 years would be most formidable, if she were the only country to have to change and had all the rest of the world as her field of operation. Under conditions of War Costs and Rehabilitation Costs being treated as business obligations there will be added to the requirements of this country those of nearly the whole of Europe and there would not be nearly the whole world, but not much more than one-half of it, to provide the markets for adjustment. *Certainly, if the Bretton Woods Agreements were not supplemented by an equitable settlement of these "non-current" transactions they would fail completely in their purpose to promote exchange stability, balanced growth of international trade, to shorten the duration and lessen the degree of disequilibrium in the international balances of members*, as are the purposes of the Fund described in its Article I.

In justice to the negotiators it must be added that most of the debt incurred for war costs has been frozen for the duration, that it is widely taken for granted that Lend/Lease balances will be wiped off, say by the return of surplus armaments after the war or otherwise and lastly, that the most urgent need of the liberated countries will be a satisfactory supply of foreign exchange to buy those commodities which they will lack during the period of rehabilitation. The shortening of the period of rehabilitation made possible by that supply may indeed be worth the future cost of interest and amortisation on the loans which help to shorten it. However, the debt to foreign countries now frozen will become liquid sometime after the war—the attempt of the Indian Delegation at Bretton Woods to have them included in the Fund's operations is a warning not to be overlooked—the cancellation of Lend/Lease balances depends largely on decisions of the American Congress which are unpredictable. Lastly, the fact that rehabilitation loans at moderate interest are more helpful than no loans at all or loans at excessive interest, does not imply that they promote equilibrium; nor can this be said of the method of just overlooking the £1,500 Millions worth of exchange, which the U.K. has surrendered to foreign countries for war purposes in the earlier stages of the conflict. Metaphorically speaking, one is tempted to say that the canal system provided for the current payments is gravely threatened by various big leaks, some of them being temporarily stopped by being frozen, and others by locks which are so constructed that after the war they will permit a substantial efflux of water. *It follows that at best the Bretton Woods Agreements—in their present isolation—represent a provisional solution, the value of which is restricted to the period of post-war transition.*

It is, however, not as a stopgap that the whole scheme has been designed. On the contrary, the regulations of the Fund provide essentially for the period that is to follow the post-war transition, i.e., for a period supposed to be free from any *fundamental* disequilibrium. As a supplement thereto the Bank's rôle is to provide for the elimination of such factors as produce a fundamental disequilibrium, but cannot be got rid of by the separate efforts of individual members. In contrast to this the agreement on the Bank does *not* provide for the elimination of such factors. It provides only for the *temporary suspension* of their weight. Its effect, unless followed by adequate supplementary agreements, would be to shorten the transitional period by way of creating a deceptive appearance as if transition had passed and a state of fundamental equilibrium had been reached. It may mislead the United Nations into applying in full the provisions of the Fund before conditions have been created which would make them workable. The solutions thus agreed upon at Bretton Woods call to the critic's mind the dictum of William Adams Brown, Jnr., on the solutions that were tried in the 'twenties in respect of the financial consequences of World War I and failed to work thereafter.

He said:[1]—" Under such conditions (viz., the World returning to the gold standard without being fully aware of the strength of the cyclical forces at work, of the depth of the economic maladjustments and of the meaning of the loss of centralised control and the new imperfections of international clearing) the international Gold Standard was a façade that masked the true nature of economic problems. It promised escape from radical economic readjustments by making hard and intractable problems appear transitory and transitional. It was a false sign-post that had made dangerous ways seem safe ".

In this writer's view it would not be fair to assume that the many well-known economists in the various delegations were not aware of the inadequacy of the regulations protecting the exchange resources derived from, and required for, current transactions against being drained off by massive commercialised " non-current " payments. Their having been well aware of it is indicated by the Article XIV of the Fund, which excludes payments for war and rehabilitation from the operations of the Fund and stresses the right of each member to retain restrictions on any kind of international payments until they are satisfied that their international payments do balance over such periods as are envisaged for the operations of the Fund. Why then have they assisted in producing such an inadequate solution ?[2] Not knowing the various delegates' mind this writer can only venture the following suggestions: Firstly, the delegates were satisfied that at this juncture the problem of a fair distribution of the costs of the war and of rehabilitation was not ripe for solution, not even for debate, neither the public nor the legislative bodies in the U.S. or the British Dominions having been in the least prepared to face the renunciation of a large part of their present credit balances which a fair settlement might possibly involve, Secondly, they realise that it was imperative at the time of the liberation of the various occupied countries to have a machinery ready which would greatly help in setting going the international exchange of goods and services and so assist them in helping themselves at the most critical period. Although a provision which " commercialises " the costs of rehabilitation may prejudice the future final settlement, still they may have thought that the time thus gained for preparing a proper settlement was worth the risk. In other words, they may have felt that at this juncture of the liberated countries being starved of foreign exchange, a bird in the hand was worth two in the bush.

IV

The Agreements also provide that the machinery established for supplying long-term facilities for the rehabilitation of war devastated countries may be used for assisting with a better access to foreign commercial capital member countries that because of economic backwardness have a poor credit standing. Such assistance would be an application of the principle that the nations form a community in which the stronger members should help the weaker ones to find their feet. Like all forms of assistance among members of a community their value depends largely on the use made of them both by recipients and donors, i.e., whether they develop into alms or into a help that assists the recipient to develop his innate faculties. The good intentions which have found expression in the " Purposes " of the Bank constitute a most appreciable progress, but the actual effect can only be judged from the practice eventually evolved. As many of the countries which may receive such assistance are at the same time those suffering from war devastation and enemy occupation with a high priority for rehabilitation loans, it may be a long time before the effects of assistance with capital other than that for rehabilitation, will be apparent. The Agreement on the Bank provides (III,1a) that its " resources and facilities shall be used exclusively for the benefit of members with equitable consideration to projects for development and projects for reconstruction alike ". The expression " equitable " has been substituted for " equal " used in a previous draft. If this paragraph means anything, it is that reconstruction is to be given preference in such cases in which the assistance of the Bank is required. The present writer, at any rate, would regard such procedure as " equitable ". Generous and helpful as they may sound, the provisions for " the encouragement of the development of productive facilities and resources in less developed countries " will be of no importance in the near future, if of any at all.

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[1] William Adams Brown, Junior : *The International Gold Standard Reinterpreted, 1914—1934.* National Bureau of Economic Research, New York, 1940, p. 389.

[2] By one of those caprices in which history occasionally delights, Professor William Adams Brown, Junior, who formulated the lesson quoted above, was made the Secretary of the Committee on Purposes, Policies and Quotas of the Fund at Bretton Woods.

What then is the answer to the questions formulated on page 166 as to the value of the Agreements? *The answer to question* (*a*) *is that the Bretton Woods scheme does not contribute to balancing, other than temporarily, the supply and demand for foreign exchange that will arise after this war. It, therefore, does not pass the main test*, unless it is supplemented by an equitable distribution of the costs of this war.

As to question (b) the answer will be apparent after the main details have been reviewed. This examination is the subject of Sections V and VI.

V

Whereas many of the detailed regulations only affect the technical efficiency of the machinery provided, some of them have a bearing on monetary and other policy. The latter to be reviewed in respect of that bearing. The regulations concerned deal with international money transfers in general, with gold, exchange rates. the gold clause, subjects reviewed in this Section V, and the distribution of the power of making decisions and, lastly, with the quotas and capital contributions to both institutions, subjects reviewed in Section VI.

The Agreements provide that without the approval of the Fund members shall not restrict the transfer of funds in " current transactions " (VIII, 2), i.e., not restrict what is mainly a discharge of obligations for goods and services received, nor manipulate their money so as to use it as a means of discrimination against other members or their nationals (VIII, 3). On the other hand, they permit the control of the transfer of funds for investment abroad and impose even the obligation to such control where an outflow of capital, as e.g., in the case of hot money outflow, would lead to a depletion of exchange reserves. Exemptions are allowed for the post-war transitional period, which is not closely defined (XIV, 2 and 4). In recent years hot money movements were permitted to absorb the foreign exchange supplied by, and required for, current transactions, and when the reserves had run dry exchange control was used to withhold payment from foreign creditors for the services they had rendered or exchange rate manipulation employed to force exports and therewith to upset at a stroke the competitive position. Moreover, the hot money funds which had unbalanced the exchange position were frequently allowed to stay abroad. By these means it was attempted to shift the burden for past neglects onto the shoulders of foreign businessmen and investors, who were penalised either by not being paid in spite of services rendered, or by being suddenly deprived of their ability to compete. The regulations of the Fund constitute, therefore, a great progress, or rather return in the direction of fair dealing. Moreover, the provisions for the periodical furnishing of information (VIII, 5) will greatly help in building up a system of warning so that Governments—and electorates pressing them—can no longer plead having been surprised by a sudden lack of exchange reserves.

Promoting balanced growth of international trade being one of the main objectives of the Agreements, promoting stable exchange rates has very properly been made one of their subsidiary objectives. Nothing helps trade more than conditions that are " stable " in the sense of being free from severe fluctuations that are unpredictable to businessmen. The regulations are so framed that the members should not *lightly* resort to the expedient of depreciating their own currencies and thus steal a march upon their fellow members not doing so. At the beginning the exchange rate is to be fixed by agreement between Fund and each member (XX, 4). Thereafter a member is free to alter its exchange rate to any necessary extent, if it applies the machinery provided for. Technically the alteration shall be made, i.e., announced by the Fund, but the latter acts only upon the proposal by the member whose currency is concerned. As regards the first 10 per cent. of change, the Fund has no right to object to the member's proposal ; for any change beyond 10 per cent. it has no right to object either if a disequilibrium does exist which is fundamental enough to require a change of the rate, irrespective of any domestic policy of the member that may have caused the disequilibrium. Only as regards a change beyond 10 per cent. which is not required by an existing disequilibrium, i.e., one that would clearly bear the marks of unfair competition, has the Fund a right to object (IV, 4 and 5, a to c). Even so, a member may insist on its purpose by withdrawing from the Fund.

Clearly, the Agreements neither impose any particular monetary policy upon the members nor prevent them from resorting to any monetary policy designed to protect their own interests. They provide, however, for a machinery outlawing arbitrary exchange manipulations undertaken for the sake of unfair competition; they put brakes on members' abuse of sovereign rights. The advocates of " rugged-disregard-of-other-nations-interest " are reminded that gains from unfettered exchange rate manipulation, such as they are, can only be reaped as long as other nations do not reply with exchange or other manipulations of their own. Alterations of exchange rates in order to be successful in improving the trade position, always require the tacit " consent " of the other nations, i.e., the latter's acquiescence in such manipulations.[1] The subjection to the consent of fellow-members through the agency of the Fund is, therefore, only an apparent not an actual restriction of rights hitherto enjoyable. The intention to restrict only the abuse of sovereign rights is stressed—indirectly—by Regulation IV, 4, 3, permitting a member to change the par value of its currency without concurrence of the Fund, if the change does not affect the international transactions of members of the Fund, as, e.g., in the case of Latin American members, who transact their foreign trade not in their national currency but in foreign currencies such as U.S. dollars. Moreover, a member whose metropolitan territory was occupied may, for a transitional time, trade under a regime of fluctuating exchange rates (XX, 4, 6 and d). During that time it may enjoy the amenities provided by the Bretton Woods arrangements and if it eventually values the unrestricted freedom to manipulate its exchange rates more than the amenities of the Fund and the Bank, it need only avoid agreeing with the Fund on eventually fixing its exchange rate and thus automatically withdraw (XX, 4, 6).

The comment on the provisions in respect of alterations of exchange rates holds true too of the regulations dealing with gold. It should be remarked that the provisions in respect of gold concern only Monetary gold; they do not deal with gold for private hoarding. There is no indication that this distinction is intentional, but it follows indirectly from the regulation (IV, 2) that no member, or more exactly, no member's monetary agencies shall buy gold above or sell gold below the par value in terms of the national currency adjusted for a prescribed trading margin, in connection with the regulation (V, 6, b) that gold mining members are free to sell newly produced domestic gold anywhere, and, lastly, in connection with the absence of any regulation requiring any particular attitude of members with regard to gold bought or sold by individuals within their jurisdiction. Members are at liberty to permit or prohibit as they like the hoarding or dishoarding of gold, at any prices, by individuals, and likewise to let their individuals deal, at any prices, with hoarders in foreign countries that permit such deals. Their own agencies may sell gold to hoarders at a price above par and buy it from them at a price below par. The pre-emptive right of the Fund (V, 6, a) is suspended in this respect by the qualification " provided the members can sell to it with equal advantage ". If hoarders are prepared to buy above par, members' sales to the Fund could not be made " with equal advantage ". The Fund is obliged to deal at par, as it can only do business with monetary agencies. It follows that in respect of the use by individuals gold is treated as a commodity.

However, in respect of the use by monetary agencies, gold is treated as an international currency, and, within limits, even as an international legal tender. In fact, it is treated as the currency of the Fund, though not issued by the Fund. The relevant regulations are as follows:—

(a) Monetary gold is given a uniform price in all member countries by way of expressing the exchange rate of each member's currency in terms of ounces of gold as a common denominator (or U.S. dollar of the weight and fineness in force on July 1st, 1944, which is only another term for $\frac{1}{35}$ oz. (fine) of gold). (IV, 1.)

(b) The Fund may make uniform changes in the price of monetary gold affecting equally the price of gold in terms of all members' currencies. An individual member can—within a 72 hours' time limit—only exempt itself from a uniform change in the price of gold, and by this way alter its own rates of foreign exchange. (IV, 7.)

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[1] The need for other nations' " consent " has been ably described in a recent publication, 1944, of the League of Nations Econ., Fin. and Transit Dep't. on " International Currency Experience " (see chapter ix, 2).

(c) Monetary gold can be dealt with only at the uniform price prescribed by the Fund, i.e., at the par value of the exchange rates of the members. No member need deal in Monetary gold, accept or release it—with exceptions described further below—but if it does deal in gold, it may do so only at par, adjusted for a prescribed trading margin. (IV, 2.)

(d) A member can acquire any other member's currency from the Fund by tendering gold at par. This right is, however, limited by the Fund's right to restrict the quantity so to be acquired, if a particular currency has been declared scarce and the Fund's release of it is being rationed. (V, 6, and VII, 3.) The Fund will exercise that right only if other measures to relieve that particular scarcity have failed, such as borrowing that currency or even requesting the member whose deposit with the Fund has fallen low to exchange its currency for gold. Therefore, as long as members co-operate, gold will buy any member's currency at the fixed price.

(e) A member may acquire its own currency from the Fund by tendering gold at par. This right is limited to the holdings of the Fund in excess of a member's quota. In other words, a member may repay the credit granted to it by the Fund by tendering gold at par. (V, 7a.)

(f) A member may tender gold at par in discharge of its 80 per cent. callable proportion to the Bank's capital, provided a call has been made.

(g) A member requested by another member to buy balances of its own currency held by the latter under the conditions of VIII, 4, may pay at his option in gold at par (or in the currency of the member making the request).

All these regulations establish—within certain limits—gold as an international legal tender for dealings with or through the Fund, but no member need make use of the facilities of the Fund.

There are the following exceptions from the right to refuse accepting, or parting with, gold:—

(i) A member whose deposit with the Fund has fallen below its quota to an extent that the Fund wishes to replenish its holding of that currency, may be " required to sell its currency to the Fund for gold ". (VII, 2, ii.) This is the only case where the Agreements provide that a member may be compelled to buy gold and do so at the fixed price. Even in this case the compulsion is not automatic, but dependent on the action of the Fund, i.e., practically on the consent of a great number of countries, including either the U.S. or the British Commonwealth. The votes of each of these two large units exceed one-fourth of the total, so that no majority vote can be formed without including either of them.

(ii) A member, of whose currency the Fund holds more than 75 per cent. of its quota—in plain English, a member who has been granted a credit by the Fund—shall, at the end of the Fund's fiscal year use a certain amount of its own monetary reserve of gold or convertible currencies to repay the credit or part of it. (V, 7, b and c.) The conditions attached to this obligation, which need not be elaborated here, make it clear that this provision only serves to prevent a member abusing the credit facilities of the Fund for the purpose of accumulating gold or certain foreign exchange. It is in the form of a bank's safeguard against a client's speculating, but does not represent an interference with a member's monetary policy.

(iii) Charges due to the Fund shall be paid in gold, provided the owing member possesses a net reserve of gold or of currencies convertible into gold to the amount of at least one-half of its quota : otherwise a part may be paid by tendering the debtor's own currency. The meaning of this provision is that, as a rule, interest on money borrowed and charges from exchange transactions shall not be borrowed from the Fund, with exceptions for members who are particularly hard up. This, too, is a provision designed as a safeguard against abuse of credit facilities, not a prescription on monetary policy.

(iv) At the beginning of operations, members pay up with gold a proportion of their total contribution to the Capital Stock of both Fund and Bank (called " quota " in the case of the Fund), the total contribution being nearly, though not quite, equal in both cases. By so doing, they provide the Fund or Bank with a stock of international currency. The proportion is 25 per cent. of the total contribution in the case of the Fund and 2 per cent. in the case of the Bank, with reductions in the case of the Fund for members whose contribution in gold would exceed 10 per cent. of their net reserves of gold and convertible currencies and for members whose metropolitan territory is or was occupied by the Enemy. As to the Fund the stock of gold so acquired is nearly as high as one-half of the maximum aggregate credits that may be granted by it, if each member in debt to the Fund were to use its line to the full, that maximum being limited to approximately one-half of the total quotas, as members possessing the other half will have a credit in their balance of payments.

Clearly all provisions as to members' rights and obligations to buy or sell Monetary gold are in the nature of using gold as an international currency, which is distinct from any member's national currency. No regulation obliges a member to any monetary policy within its own realm, with the one exception of the presumably rare case of a member whose deposit with the Fund has been declared scarce and may be requested to exchange its own currency for gold. Nor is there any other provision which would require a member to pursue any particular monetary or any other policy or, as long as it is restricted to the latter's own province, prevents members from doing so. The members are completely at liberty to expand or contract their own money, to balance or unbalance their budgets, to invest or disinvest at home; in short, to pursue any domestic monetary policy they think fit. The essence of the Gold Standard, in whatever form, is that gold and national currency are convertible into each other at fixed prices, and that the change of the monetary gold holdings leads automatically to either expansion or contraction of the money issued, although the automatism may be mitigated by various gears of transmission and shock absorbers. The Agreements of Bretton Woods provide neither compulsory convertibility into gold of a member's own currency nor any link of his money issues with his reserves of gold. Clearly it imposes no Gold Standard of any kind on members. In fact, it provides for no monetary standard at all; it only imposes on members fair play in International dealings, especially non-interference with an individual's obligation to pay a foreigner, once that individual has been allowed to enter into such obligation in exchange for the foreigner's services. There is, therefore, no foundation for either criticising or praising the Agreements because of their alleged re-introduction of the Gold Standard. They do not re-introduce it.

The provisions permitting Gold to serve as an international currency facilitate the clearing of international payments. They also assist the monetary policy of members, inasmuch as they aid in preventing other members using as their monetary reserve the former members' national currencies. It may well be said that if there had been no medium available that could serve as international currency, the Fund would have to create one. At this juncture, gold serves this purpose better than any national currency appropriated as international currency, or any created by the Fund. After all, gold is established by general acclamation and practice as the international currency, and only by government interference restricted in so serving.

No answer can, however, be given to the question whether it was wise not to link the members' monetary policy with the movement of their gold reserves, or whether it would have been wiser to do so. Very little scientific light has been shed upon this problem so far, and we had better admit that economic science is quite in the dark in this respect. At present, it is more a question of faith, whether to prefer unfettered money management by governments or management tempered by a reserve the available quantity of which does not easily respond to man's changing moods or ingenuities.

A most remarkable feature of the Agreements is the re-introduction of the gold clause. The 'thirties saw a wholesale nullification—by administrative or judicial actions—of the gold clause, by which private contracting parties had bound themselves to maintain the value, in terms of gold, of their respective rights and obligations whatever the change of the purchasing power of the money in which they were expressed. Gold clauses were particularly numerous, in fact the rule, in loan contracts drawn up in the U.S. In contrast to the wholesale nullification of the Gold Clause in the U.S. and elsewhere, the Bretton Woods Agreements provide for a clause (IV, 8) adjusting the currency assets of both the Fund and the Bank against any change in the par value, i.e., gold value of a member's currency. In the case of a uniform change of par values, i.e., one engineered by the Fund itself, the Fund and Bank may waive that right. On the other hand, the clause extends beyond the change of par values. It also includes a protection of both the Fund's and the Bank's assets against " a significant depreciation of a member's foreign exchange value in the member's own territory ". The meaning of this extension might have been expressed more clearly. This writer interprets it as a protection against loss on deposits of a member who is unable to prevent its own currency being dealt in in its own country against foreign currencies at a much lower price and on substantial scale, so that the purchasing power of those deposits is very much lower than would appear from the official par value of the currency. Conditions of this kind exist at present in China. No adjustment for a currency being dealt with at higher prices than the official has been provided for.

On the face of it, those clauses apply only to the Fund's or Bank's assets, and not to a member's own holdings of another member's assets. However, a member has the right (VIII, 4), in respect of any recently and " honestly " acquired currencies of another member, or any honestly required liquidation, to request the latter to exchange them for the former's own currency (or gold at the requested member's option) and thus to protect itself, to a certain extent, against losses on assets against currency depreciation by another member. Lastly, if the clauses protecting the assets of the two International institutions are valid, it will be difficult by judicial action to nullify similar clauses in private arrangements. A provision by which a country making use of its power to change the value of its currency is prevented from deriving profit or loss from such action in respect of past services, is more likely to be conducive to fairness in international dealings than not. At any rate, if copied in private arrangements, the Gold Clause may prove to be of far-reaching importance.

VI

The organisation of the two institutions to be set up, the Fund and the Bank, is of great interest. Firstly, it has to ensure the smooth working of an international organisation composed of men drawn from many countries with various loyalities, dealing with matters which, in modern times, have been regarded as the exercise of sovereignty, and doing so in the very period which had made a cult of sovereignty. There is no more difficult person to deal with than he whose actions are swayed by his regard for his prestige. Secondly, it sets an example—for better or worse—to the organisation that may be adopted by other institutions serving international co-operation. Bretton Woods had to break new ground. Moreover, the governments there represented were faced with the particular difficulty that this war has found the smaller nations unable to defend themselves against any big nation, and has laid the main burden of conducting war on the latter. This war has demonstrated a great inequality of functions performed by various countries to a world claiming equality of rights for each group constituted as a country irrespective of size, actual functions and services. As a consequence, delegates were confronted with a tendency to allocate all rights to the big nations and to make the smaller ones mere objects of their considerations, though such considerations may be most benevolent towards the smaller ones. Bretton Woods resorted to a compromise of the two tendencies towards " equality of rights " and " inequality of might ". The solution is nearly, but not wholly, alike in the case of the Fund and the Bank, but similar enough in both instances to justify limiting a review to the organisation of the Fund as an example for both. The essence of the set-up is, firstly, that each member is entitled to cast its vote in making decisions, but that votes are not counted but weighed (XII, 2). This representation of all members called the Board of Governors, resembles a legislative assembly instructed by constituencies with plural votes. Secondly, there are the Executive Directors, functioning somewhat like a Government, drawn from the Lords and from the Commons in equal numbers. Five Executive Directors are appointed as of right by each of the five biggest nations (i.e., the five nations with the largest quotas, viz., U.S., U.K., U.S.S.R., China and France, the " Lords " as it were), not more than two are appointed as of right—in certain conditions—by nations out of whose funds credits to other members are supplied—this applies only to the Fund also " Lords ",—two more are elected by the Latin American members, the " Commons ", and five more elected by all other members, also " Commons ". In the case of the Bank, the Latin American Members do not form a separate electorate. The Executive Directors are responsible for the conduct of the general operations and are assisted by a kind of civil service with a Managing Director as its head, nominated by the Executive Directors (XII, 3). Thirdly, a number of " big " members have a right to veto certain decisions. In a few cases the small members have a veto too, inasmuch as for some decisions the consent of the particular member concerned is required. Votes will be weighed according to the votes allotted to the members appointing or electing the Executive, but as they are cast by each Executive Director in one unit (XII, 3, i), the smaller nations can make their influence felt at best by influencing the decision of their own group instructing the elected Director, whereas each of the big nations instructs its own Director. The voting power of each member corresponds to its proportion in the capital (in the Fund " quota " stands for capital) but is slightly adjusted in favour of the small nations at the expense of the larger ones. By the device of giving to each member 250 votes plus one vote for each $100,000 quota (or Capital Stock) (XII, 5), all nations with a quota of less than 2½ per cent. gain slightly and those with more lose proportionately. Still, the economically more powerful members retain enough votes to veto many decisions and even to sway them, provided a few of them act in concert. The following list shows the main distribution of power in the Fund:—

Member of the Fund	*Quota*	*Voting Power*
	% of Total	
U.S.A.	31.1	27.8
U.K.	14.7	13.3
British Commonwealth (acting in concert, excluding India)	22.0	20.8
British Commonwealth (acting in concert, including India)	26.5	25.1
U.S.S.R.	13.6	12.3
France	5.1	4.8
All Latin American Members	5.5	9.7

It will be seen that no majority at all is possible against the U.S. and the British Commonwealth, including India, acting in concert, or against the two excluding India, but with the addition of one of the three Western European members. Four-fifths majority being required for changes in quotas (III, 2) and for admitting new Executive Directors following the admittance of new members (XII, 3, b), the U.S. and the British Commonwealth, the latter acting in concert, have each of them vetoing power. The smaller members are protected by their consent being required for a change of their own quota (III, 2). Majority of votes including the consent of each member having 10 per cent. or more of the quota being required for any uniform change of par values (i.e., uniform change in the price of gold) (IV, 7), the U.S., the U.K., and the U.S.S.R. have each of them vetoing power. These three acting in concert can, moreover, decide on any such uniform change, as their votes exceed one-half of the total. South Africa, on the other hand, having before this war accounted for about one-third of the total gold production, has little say in this matter, unless she can influence the vote of the U.K., her own voting strength being only 1.2 per cent. However, the smaller nations have the—doubtful—protection of exercising their right to exempt themselves from such a uniform change (IV, 7). Modifications of the terms of the Fund—with the exception of three vital terms for which unanimous acceptance of amendments is required—will take effect after they have been adopted by a majority of votes of Governors and, moreover, been accepted by three-fifths of the members having four-fifths of the votes and a certification to this respect has been sent to all members (XVII). The U.S. and the British Commonwealth have each of them, therefore, vetoing power and so have a number of the medium sized nations, if the latter act in concert. Two-thirds of total voting power is required for publishing a report made to a member regarding its monetary or economic conditions, i.e., practically the publication of a censure (XII, 8). Only a majority of Governors, i.e., of members, provided they also represent the majority of voting strength, can expel a member (XV, 2, b). This provision gives a veto both to the big members if they act in concert and to the small nations; twenty-three of them may represent only 11.5 per cent. of voting strength, but may constitute the majority of Governors. No unanimous vote, but only 65 per cent. of the total of initial quotas (or capital subscriptions) is required for the entry into force of the Agreements, so that non-ratification by one of the big members does not require the scrapping of the whole effort (XX, 1). A unanimous vote of the Executive Directors, of whom as will be remembered not less than five are appointed and, therefore, instructed by one of the big members, is required for emergency action suspending certain articles and four-fifths of total voting power for an extension of such actions, both provisions giving vetoing rights to the big nations. (XVI, 1). Lastly, in respect of certain decisions affecting the right to use the credit facilities of the Fund, the votes themselves are adjusted in favour of the creditor members and against the debtor members by adding to the votes of the creditors one vote for each $400,000 net credits provided from their deposits and deducting from the votes of the debtors one vote for each $400,000 net debits taken by the latter, with a maximum adjustment of approximately 25 per cent. on either side (XII, 5, b).

In view of so much power and many amenities and obligations being dependent upon a member's quota (Fund) or Capital Stock (Bank), the question is pertinent, by which method the various shares are to be allocated. The task of the reviewer in this respect is made difficult by the secrecy which surrounds the principles and formulæ, if any, employed in assigning to the members their various positions. He can do little more than draw attention to the fact that the same or nearly the same gradation is made to serve for very different functions.

The " quota " in the capital of the Fund is used in allocating a fixed capital contribution, out of which short-term credits in current transactions are granted and losses, if any, borne. However, it is also used in allocating the maxium credit lines that will be available to the debtors. One would think that the capital contribution would be largely adjusted to the economic strength and potential exchange reserves of a member, whereas the credit lines to the periodical fluctuations in current transactions. This same quota is, thirdly, used—with slight adjustments—in allocating the power to make or wreck decisions, and its importance in this respect is even enhanced by the many requirements of qualified majorities enumerated in the foregoing paragraph. The principal business of the Fund is to establish " fair dealing in International intercourse ", whereas supplying a machinery for short-term credit facilities is only a minor responsibility. The latter will work only if current transactions and business investments are equilibrating over the run of a few years, but if they do, the various countries will not lack short-term facilities in the market, apart from the period of transition. In respect of " fair dealing ", however, the economically powerful nations cannot be supposed to be " intrinsically " better than their smaller brethren. The record of some of them is certainly worse than that of some smaller nations. Ability to supply funds need therefore be no dominant source of voting strength. Still, one is entitled to hope that the new responsibility accepted with the greater voting power will make the big nations exemplars of fairness. Fourthly, the capital contributions to the Bank—and these may be real risk bearers—have also been based upon and made nearly equal to the quotas in the Fund—the small alterations being due to bargaining—and so has, fifthly, the voting power in the Bank. Not the slightest logical reason exists for linking with a country's need for periodical overdrafts in current account its capacity to supply capital for other needy countries in non-current accounts or its ability to show wisdom in the distribution of capital to be invested in non-current account. There was, of course, the powerful practical reason of simplicity at a juncture when time was pressing and no sets of appropriate formulæ were ready, and there was, too, the other practical reason, no less weighty, that the big nations mainly responsible for the liberation of the world could not be made to bind themselves to co-operation with the host of smaller nations without a compromise in their favour in respect of the distribution of voting power. Once both Fund and Bank are working it will be easier to see the needs and responsibilities in their proper perspectives, and to evolve formulæ that determine the magnitudes of rights and obligations on a reasonable basis, obviating thereby the need for self-assertion.

The Bretton Woods Agreements so far concern only the intercourse of the 45 United Nations (including Denmark). The Neutrals (including, e.g., Eire) and Enemy Countries may, in course of time, constitute a considerable element of outsiders. Still, it is possible that with or without admittance of new members the standards set by the Fund and the Bank will be adhered to by most of the other nations of importance.

The reviewer is now in a position to reply to the second question formulated on page 166. "How far do the Agreements assist in introducing into the dealings of nations fairness and respect for other peoples' interests in the place of strife and self-assertion ? " The answer is that they do so very substantially. However, this reply requires a qualification inasmuch as only future practice can show whether the power accorded to the big members is used beneficially or abused.

The judgment on the Agreements as a whole may be formulated as follows:—

> *They constitute a step forwards in the right direction, but too short and timid a step to reach the goals set before the nations and enumerated in the Articles I of both Fund and Bank. Further steps will be needed in the very near future.*

The
Origins of the Bank Charter Act, 1844

By J. K. HORSEFIELD

"*. . . there follows much of a circumlocutory nature connected with the Inherent Principle of Equipoise, and so forth.*"

"*That can fittingly be reserved for our leisurely delectation at some future date*" *put in the Highest.* "*Insert your chopstick in the solid meat, Tso Paik. What have we got to do?*"
Kai Lung unrolls his mat.

I

THE Bank Charter Act of 1833 renewed the privileges of the Bank of England for 21 years from August, 1834, but included a "break clause" by which notice to determine the Charter could be given within six months after August, 1844. It was this clause which gave rise to the much better-known Act of which the centenary has just been celebrated.

Had the Bank lived up to the principles expounded by its spokesmen before the Committee of 1832[1] it is probable that the full period of 21 years from 1833 would, as on previous occasions, have been allowed to elapse without Parliamentary intervention. But crises developed in 1835–6, and more emphatically in 1839, for which the Bank was, by contemporaries, held much to blame. Public opinion was also shocked that help had had to be sought from France in order to maintain the convertibility of Bank Notes.[2] Consequently, the Directors' activities were hotly criticised, and the application of the "break clause" came to be generally expected.

Both Parliament and the City were, however, completely fogged about the real nature of the problems involved and the remedies possible. A Secret Committee on Joint-Stock Banks (1836—1838) and a Select Committee on Banks of Issue (1840–41) produced only masses of detailed evidence, from which the conscientious student of banking reforms could have deduced almost any remedy he cared, but which the Committees themselves found it impossible to digest. Progress would scarcely have been possible except for the fact that when the year 1844 arrived the Prime Minister holding the reins had thought extensively, if not very clearly, about currency reform, and was prepared to act.

Some indication of the way in which Peel's mind worked may be found by studying his votes and his recorded questions in 1811, 1819 and 1840.[3] That by the last-mentioned year he had found a theory which satisfied him has been plausibly argued from the fact that he attended only those meetings of the 1840 Committee at which the proposals of the Currency School were expounded. When, at the end of that Committee's list of witnesses, Thomas Tooke appeared as the sole champion of the Banking School, to which he had recently been converted, Peel was missing. He had heard all that he wanted. By the time his next Ministry was formed, in September, 1841, he was ready to apply the doctrines which he had absorbed to the problem of the Bank.

The arguments used to support the Bill which he introduced, and which became without amendment the Bank Charter Act of 1844, resembled in close detail those used before the 1840/41 and earlier Committees by the most outspoken critics of the

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[1] In particular, the enunciation of the "Palmer Rule", as to which see below, p. 183.

[2] In 1839 the Bank borrowed £2 million from the Bank of France, through Baring Brothers.

[3] He voted against the Horner Resolutions in 1811; spoke for the Bullionist Principles in 1819; and was converted to the Currency Theory some time after 1833. (McLeod, *Theory and Practice of Banking* (3rd Edition), II, p. 338.)

Bank. From this fact it has been customary to deduce, not without some show of probability, that the Act was something of a strait-jacket, fastened on a reluctant (though subsequently complacent) Bank by the efforts of the Currency School through their ally, Peel. But a discovery made by Sir John Clapham, during researches at the Bank of England for his recently published *History*, has thrown a new and interesting light on these transactions.

It was already known that in January, 1844, Peel invited the Governor and Deputy Governor of the Bank (William Cotton and J. B. Heath) to confer with him. Sir John has now disclosed[4] that on February 2nd these two submitted to Peel an outline of their own proposals. There is apparently no evidence to shew that they consulted their colleagues before drafting their memorandum, which may not, therefore, have represented the official view of the Bank, though as we shall see there had been discussion in the Court along the lines followed. But the revealing fact emerges that this memorandum contains almost all the essential features of the 1844 Act: the prohibition of any further banks of issue; the limitation of existing note-issues to their then size; the division of the structure of the Bank between an Issue Department and a Banking Department; and the limitation of the notes issued against securities to a fixed sum, any further issues to be backed by bullion. It included also a proposal that extensions of the fiduciary issue should be permitted, if and when the need arose, on the sanction of three Ministers of the Crown. All, excepting the last of, these suggestions were embodied in the Act; something like the last became law eighty-four years later.[5]

There can, indeed, be only one explanation of these developments. At the conference in January Peel must have discovered that the ideas which he had been revolving were already present in the minds of the Governor and his Deputy. The Act itself, therefore, took shape, not as a framework imposed from outside, but as an attempt by the bank to find for itself a short-cut to currency management. (It was, as we shall see, only the latest of a series of such attempts.) Peel's Cabinet memorandum, setting out three courses which could be taken to deal with the monetary problem, was accordingly phrased so as to influence his colleagues towards a course which the Bank had proposed; and which left it substantially the only issuer of Bank notes, separating these functions from banking.[6] It is no wonder that Tooke, writing in 1857,[7] remarked on the " cordial approbation and co-operation " accorded to Peel's scheme by the Bank.

But this clarification of the relationship between Peel and the Bank in the drafting of the Act goes only a small way towards elucidating its origin. Two questions at once spring to mind: how did the Bank come to hold opinions so widely different from those which it had earlier espoused? and how did these opinions give rise to the complex machinery of the Act, with which their connection is far from obvious? In a short article there is no space to do more than outline the answers to these questions, but the attempt is perhaps worth making afresh in the light of Sir John's discovery.

In what follows the attitude attributed to the Bank is that to which its official actions appear to have conformed. We are obliged to ignore the more subtle and often more enlightened views of the individual Directors who from time to time shewed a clearer appreciation of their problems, and of the real issues at stake. The reader will therefore understand that the picture presented belongs to the Impressionist, and in no sense to the pre-Raphaelite, School.

II

It is in 1783 that we first find clear traces of banking policy actuated by a theory of currency. In that year the Bank temporarily restricted credit in order to alleviate a drain on the foreign exchanges. Between 1793 and 1797 other measures, including the rationing of discounts, were applied to similar situations. From these experiences the spokesmen of the Bank in 1797[1] deduced that normally the circulation of the Bank affected prices and consequently the foreign exchanges; but that in times of internal crisis the effects on prices were likely to be counterbalanced by changes in the velocity of circulation. They accordingly suggested that a drain of gold caused by adverse foreign exchanges could be checked by a contraction of the Bank's circulation, leading to a reversal of the balance of payments; but that for an internal drain the remedy was not the contraction, but the expansion, of credit in order to maintain confidence.

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[4] Clapham: *The Bank of England: A History*, II, pp. 178–9.

[5] Currency and Bank Notes Act, 1928, s. 8.

[6] Summarised by Andreades, *History of the Bank of England*, pp. 407–410.

[7] *History of Prices*, V, p. 529.

[1] See evidence of Daniel Giles and Samuel Bosanquet to the House of Commons Committee on the Outstanding Demands of the Bank of England.

These views, expounded and clarified by Henry Thornton, Lord King, and the authors of the Irish Banking Report of 1804, pointed the safe but arduous way to currency management, through constant watchfulness and intelligent interpretation of current events. Had not the Suspension of Cash Payments intervened, cutting off the Directorate for 24 years from contact with the foreign exchanges, it is possible that monetary theory during the first half of the nineteenth century would have developed under the leadership of successive Governors of the Bank, instead of mainly by way of protest against its obscurantist attitude.

As it was, however, the Bank relapsed into passivity, under the comfortable delusion, based on Adam Smith,[2] that nothing could go wrong so long as discounts were confined to satisfying the legitimate needs of trade. Worse still, the artificiality of the currency situation, due to the Restriction of Cash Payments, affected even economists who clearly saw the fallacy of the Bank's position. Thus, the Bullion Report, accurate and lucid on the Quantity Theory, allowed itself to conclude that "no sufficient remedy for the present, or security for the future, can be pointed out, *except* the Repeal of the Law which suspends the Cash Payments of the Bank of England".[3] It was this doctrine, over-simplified into a blind faith in convertibility as a panacea, which formed the next stage in the Bank's intellectual pilgrimage.

For a time, however, the Court refused to accept such revolutionary views. Even as late as 1819 there was a strong party in the Court which continued to maintain that the Bank's circulation had nothing to do with the foreign exchanges; and expressed this view in an often-quoted resolution, of which the material section runs: "This Court cannot refrain from adverting to an opinion strongly insisted upon by some, that the Bank has only to reduce its issues to obtain a favourable turn in the exchanges, and a consequent influx of the precious metals; the Court conceives it to be its duty to declare that it is unable to discover any solid foundation for such a sentiment".[4] I have suggested elsewhere that it is perhaps not necessary to take at their face value the ostrich-like sentiments expressed in this sentence[5]; but evidence given to the Committee of 1832 shewed that the subsequent change of heart amongst the members of the Court was sufficiently well marked to warrant the formal rescission in 1827 of the offending resolution.[6]

For our present purposes it is immaterial at what date the enlightenment arrived. It is sufficient that somewhere between 1810 and 1827 the Bank came to accept the view which it attributed to the Bullion Committee. With its genius for over-simplification, it then devoted its energies to safeguarding the convertibility of its notes. This had the convenient consequence that it could disregard the embarrassing problem of the country banks' note-issues, which had multiplied greatly since the days of the eighteenth century Governors, and which the Bank now thought it could not control.[7] Unfortunately, its newly-won belief was no sooner shaped than shattered, for the crisis of 1825 demonstrated that a convertible currency was none the less capable of being issued to excess and of feeding a commercial cataclysm.

What happened next is nowhere very clear; but, endeavouring as best we may to interpret the evidence offered to the Committee of 1832, we may guess that the Court persuaded itself that the real answer was the current variant of the Bullion Committee's theory, associated with the powerful name of David Ricardo. This early version of the Currency Principle substituted for the test of convertability the test of correspondence with an all-metallic currency. In Ricardo's view, a country which had no paper-money could never suffer a serious commercial crisis from adverse foreign exchanges, because the drain of gold abroad would rectify them before disaster occurred. From this he deduced that a currency which acted in all respects like gold, whether it consisted of coins, paper, or both together, would achieve the objects of credit control.[8] Accepting this doctrine, the Bank translated it into the practical policy of the "Palmer Rule", named after J. Horsley Palmer (Deputy Governor 1828–30, Governor 1830–33), who first enunciated it. This was the procedure by which the securities in the Bank's possession were to be held at a fixed level, whilst its gold, and therefore its notes, were allowed to fluctuate upwards or downwards in accordance with public demand for the metal.[9] This was, of course, a rough and ready way of achieving Ricardo's aim, and had for the Bank the all-important merit of simplicity. What it might have accomplished, however, remains unknown, since the Bank scarcely ever succeeded in following it. We have no room to discuss here the rather tangled history of the years 1835–39, but the net result of their problems was that the Bank's securities fluctuated so considerably that it seemed as if they, and not the note-issue, formed the variable factor. It is unnecessary to assume that this was due to wilfulness or bad management on the part of the Bank. Sooner or later—and in the event sooner rather than later—the Bank's business as a borrower and lender of deposits was bound to involve it in variations in its securities; and during the difficult years at the end of the thirties these variations were severe enough to nullify completely its attempts at currency control by the simple tactics of the "Palmer Rule".

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[2] *Wealth of Nations* (Cannan's edition) I, p. 287.

[3] *Bullion Report* (Cannan's edition), p. 68. (My italics.)

[4] Secret Committee on the expediency of the Bank resuming Cash Payments. Second Report. Appendix.

[5] ECONOMICA, May, 1940, p. 168. It should be remembered that in the same Resolution reference was made to the necessity for building up adequate reserves to ensure *convertibility*.

[6] Committee of Secrecy on the Bank Charter, 1832, QQ. 2075—2080.

[7] See, e.g., evidence of Horsley Palmer to 1832 Committee, Q. 91.

The final stage between this disappointment and the beliefs cherished by Cotton and Heath seems to have been an awakening to these elementary facts. From them, still pursuing the will-of-the-wisp of a fool-proof method of monetary management, the Bank was led on to an acceptance of the doctrine advocated by S. J. Loyd (later Lord Overstone) that the reason why the "Palmer Rule" broke down was because it was inappropriate to an institution engaged in deposit banking as well as in note-issue.[10] Hence the way out was to separate these functions.

Once this principle was accepted, it was, of course, no more than a step from the "Palmer Rule" of 1832 to the Fiduciary Issue of 1844. Both proposals incorporated a fixed volume of securities, and both envisaged the Bank passively receiving and issuing gold, pound for pound, against the notes above this limit.

The main principle underlying the 1844 Act can therefore be briefly but compendiously described as the application of the Currency Theory to the "Palmer Rule". Its exponents held, with Ricardo, the rigid Quantity Theory explanation of the link between the volume of currency, the level of prices, and the level of the foreign exchanges; they ignored, again with Ricardo, the influence of deposits on prices and gold movements, and influences from natural causes (such as bad harvests) on the rate of exchange. So simplified, monetary management presented itself as a problem merely of tying the note-issue to gold.

Thus, during the whole period from 1810 or earlier to 1844 or later, the Directors cherished a delusion that their proper policy could be condensed into a simple and automatic rule. During most of the period of Cash Restriction they thought it was enshrined in the principle of satisfying the needs of trade. About 1820 an imperfect appreciation of the Bullion Report led them to think that convertibility was the sovereign touchstone; but just when the admirable simplicity of this conception had won acceptance, it too was shattered by the crisis of 1825. The automatism of the "Palmer Rule", which succeeded, breaking down in its turn, the Governors developed the final formula which became the Act of 1844. Always fruitlessly seeking for an easy escape from the unwanted burden of monetary management, which their 18th Century predecessors knew could not exist; and being repeatedly disappointed in their simple-minded hopes; the Bank did little more for forty years than flirt with fundamental fallacies in hopes of avoiding that active responsibility for currency control which, on their behalf, the aged Thomas Hankey was vigorously repudiating even thirty years later on.[11]

III

We come now to consider the origin of the specific provisions made in the Act for the mechanism of issue. We may distinguish four principal subjects: the Separation of the Departments at the Bank; the establishment of the Fiduciary Issue; the control of the Country Bank Notes; and the publication of accounts.

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[8] *Proposals for an Economical and Secure Currency*, especially Section IV.

[9] 1832 Committee, QQ. 72, 2460, 2394, 2395.

[10] *Reflections suggested by a Perusal of Mr. J. Horsley Palmer's Pamphlet on the Causes and Consequences of the Pressure on the Money Market* (1837) in *Tracts*, p. 6.

[11] *The Principles of Banking*, especially Preface to the Second Edition (1873) *passim*.

The few remaining clauses dealt with purely formal details, such as the continuation of the Bank's privileges, and for the most part followed well-established precedents; these require no comment.

The importance attached to the Separation of the Departments is clearly indicated by its appearance as Clause 1 of the Act. It seems to have been derived from Ricardo's posthumously-published *Plan for the Establishment of a National Bank* (1824),[1] in the first paragraph of which it is roundly asserted: "The Bank of England performs two operations of banking which are quite distinct and have no necessary connection with each other: it issues a paper currency as a substitute for a metallic one, and it advances money in the way of loan to merchants and others". In the second paragraph he argues that these two functions might be carried on by two separate bodies, and from that postulate develops a proposal for National Commissioners to manage the issue, leaving deposit functions to the Bank.

Ricardo's scheme thus combined two separate threads of reforming thought—the assumption of note-issue by a Government agency, and its total separation from deposit banking. The first of these can be traced back at least to 1803.[2] In Ricardo's own writings it appears as early as 1815[3] and it met with a good deal of support from individual critics of the Bank whatever their other ideas. Some such question and answer as the following is of frequent appearance in the Minutes of both the 1832 and the 1840/41 Committees. The witness on this occasion was George Grote, a London banker, before the 1832 Committee (Q. 4747): ". . . do you think it would be proper for Government itself to become the issuers of paper, in preference to the Bank?" *Answer:* ". . . I am inclined to think that it would be better to leave the Directors to manage the circulation as they now do, in preference to having national commissioners appointed for the purpose . . ." Such a proposal for complete segregation, which to-day is a political rather than an economic issue, represented perhaps in the early nineteenth century no more than the protest of a logical mind against a typical British compromise ; or at most a deep-rooted mistrust of the Bank. It was rejected by Peel, in his speech introducing the 1844 Bill, but only on the ground that it was now too late to think about a State issue.

For our purpose it is the second facet of Ricardo's suggestion which is important —the idea of separating the note-issue, not from the Bank of England as such, but from deposit banking in general. This was not explored by the 1832 Committee, and it appears not to have found a publicist until 1837, when Col. R. Torrens published a pamphlet in which he proposed a London bank, with no country branches, issuing notes only against bullion, and doing no deposit banking.[4] At that time he appears to have thought that such a scheme would suffice to ensure that the circulation of the country banks would also fluctuate as if their notes were coins—the desideratum of a true Ricardian—but by 1844 he had come to feel that control of the country banks issues was also necessary to achieve this result.[5] (On which, see below.)

From 1837 the Separation of the Departments immediately became a live issue It was taken up by Loyd, who advocated it because "The principles upon which these two branches of business ought to be conducted are perfectly distinct and never can be reduced to one and the same rule".[6]

In the following year it was discussed inside the Bank. Someone—Acres suggests it was G. Warde Norman[7]—proposed to the Court that the Chief Accountant and the Chief Cashier should report on a plan for separation. This proposal was negatived, but Norman proceeded to expound the plan in a pamphlet, which was printed for private circulation in 1833, though it was not published until 1838.[8]

By 1840 the pot was boiling vigorously. The Committee of that year were very interested in the scheme, and tried it out on most of their witnesses. J. B. Smith, banker at Manchester,[9] Richard Page (=Daniel Hardcastle, pamphleteer on banking)[10] and G. F. Muntz, M.P.,[11] still hankered after Government issues, but others, such as W. R. Wood, a Manchester merchant[12] examined between them, definitely preferred a separation of the departments. Then came Palmer, who " had not considered Separation in detail " but " never expected any benefit " from it.[13] Norman, the next witness, rendered cautious, no doubt, by the Bank's experiences in 1839, replied to a question on the best remedy for defects in the circulation by suggesting no more than a separation of the *accounts*. " A separation of the *departments* " he thought " under actual circumstances could not be maintained ".[14] This suggests that he, at least, was not blind to the point, subsequently taken by critics of the Act, that a rigid Separation of the Departments might bankrupt the Banking Department first. But the Committee continued to ask questions about it, and eventually obtained a whole-hearted endorsement of separation from Loyd, who progressed during his evidence from a re-assertion of the importance of separating the incompatible functions of note-issue and deposit banking[15] to a specific suggestion that the Bank could be divided into two departments.[16]

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[1] Reproduced by Andreades, *op. cit.*, pp. 417–427. An earlier version, dated 1823, is summarised by Rist, *History of Monetary and Credit Theory* (English translation), pp. 177–179. There seems to be no foundation for Joplin's complaint that Ricardo pirated the idea from him. (*An analysis . . . of the currency question* . . . (1832), p. 146.)

[2] W. Howison, *An Investigation into . . . the Circulation of Paper Money* . . ., p. 29.

[3] *Letters to Malthus*, 10th September, 1815 (Bonar's edition, p. 89).

[4] *A letter to Lord Melbourne on . . . Bank Reform*, pp. 49–60 (Summarised by Elmer Wood, *English Theories of Central Banking Control*, 1819—1858, p. 112).

[5] *An Inquiry into the . . . Proposed Arrangements for the Renewal of the Charter of the Bank of England* . . . (2nd edition), pp. 39–43.

[6] *Tracts*, p. 63.

[7] *The Bank of England from Within*, II, p. 469. (Although a Director of the Bank for 50 years, Norman never became Governor.)

[8] *Remarks upon some prevalent errors with respect to currency and banking.*

Meanwhile, as Sir John Clapham now tells us[17], the issue had become a live one inside the Bank itself. A report on banking re-organisation made to the Court in March, 1840, by J. H. Pelly (Deputy-Governor, 1840–41, Governor, 1841–42), Norman and W. Cotton (Deputy Governor 1841–42, Governor, 1842–45) suggested that no great change was needed; but was met by a series of amendments which, though then lost, eventually emerged as bank policy. One of these amendments proposed the separation of the departments. A year later, in May, 1841, Pelly, Cotton, J. B. Heath (Deputy Governor, 1842–45, Governor, 1845–47), Norman and J. Morris (Deputy Governor, 1847, Governor, 1847–49) were appointed as a Committee to discuss the division of the circulating and banking accounts, and although no report has survived, the fact that Cotton sent Peel in January, 1844, what was termed by him " a sketch of the division of the bank " suggests that the Committee reached some definite conclusion in favour of this measure. This was amplified at the beginning of February, 1844, by the memorandum by Cotton and Heath to which reference has already been made; and by the time the Chancellor of the Exchequer sent the usual official letter to the Bank in April,[18] the project had taken final shape. It was, of course, an obvious development of the Ricardian idea that for practical purposes it was unnecessary to control deposits, but essential to manage the note-issue.

IV

Turning to the second main subject of the Act, the Fiduciary Issue, we may ask in the first place from what source the conception of the Issue itself was derived, and secondly who or what suggested the figure of £14,000,000 at which it was originally fixed.

The Fiduciary Issue can be regarded either as a special case of a general tendency to seek to limit the note-issue, or as a piece of mechanism designed to tie notes to gold. The former interpretation leads us back at least to 1797, when Sir Francis Baring was advocating " limiting the amount to which the company [i.e., the Bank of England] shall be permitted to issue their Notes . . ."[1] because of the fear of inflation. To this view there never lacked antagonists. Thornton, for example, in the same year (1797) deprecated any artificial limitation for the same reasons as subsequently gave rise to the agitation for a Relaxing Power, *viz.*: " the Quantity of Notes which it may be proper at any time to issue [depends] much on the State of the Public

[9] Q. 365.
[10] Q. 970.
[11] Q. 1051.
[12] QQ. 625—629
[13] Q. 1603.
[14] Q. 2161. (My italics.)
[15] QQ. 2732—2736.
[16] Q. 2878.
[17] *Op. cit.*, II, pp. 173–5.
[18] Reprinted, Gregory, *British Banking Statutes and Reports*, I, p. 117.

[1] *Observations on the Establishment of the Bank of England* . . ., p. 73.

Mind ".[2] These opposing points of view, increasingly elaborated and sophisticated, recurred throughout the next half century. But again it is to the narrower aspect of the proposal that we must confine our attention.

There can be no doubt that, thus viewed, the Fiduciary Issue was directly descended from the conception of a fixed volume of securities which was the essence of the " Palmer Rule ". It is true that in 1832 Palmer so envisaged a steady level of securities held against notes and deposits together, but the adoption of the Ricardian notion that deposits might be disregarded led inevitably to the association of the fixed level of securities with the note-issue, leaving the fluctuating bullion reserve to act as backing for the remainder of the issue. This was so essentially similar to the " Palmer Rule " as to cause no difficulty to the contemporary observer.

As such, it received a wide measure of support. It was, perhaps, to be expected that Norman, preaching the doctrine that bullion should back notes only, not notes plus deposits, actively supported the plan before the 1840 Committee.[3] But even so pig-headed a witness as Richard Cobden, to whom the Bank of England was as a red rag to a bull, was eventually driven to answer " Precisely " to the question: " Do you mean . . . a certain fixed amount [of notes] to be issued on security, the remainder to be issued against gold and vary with it ? "[4] Tooke, again, backed the scheme, on the original ground that it would minimise variations in the rate of interest.[5] And even those witnesses who preferred National Commissioners to the Bank of England visualised some kind of a Fiduciary Issue as the appropriate mechanism. J. B. Smith, for example, conceived of such an issue to the tune of £20 million, additional to a supposed £10 million, more or less, issued against bullion.[6]

The origin of the figure of £14,000,000 is less clear. There was a persistent contemporary belief that it was connected in some way with the Bank's capital, which was—and is—£14,553,000. This was denied at the time,[7] but Sir John Clapham's discoveries suggest that there may have been something in it.

One of the 1840 amendments to the report by Cotton, Norman and Palmer, already mentioned, suggested that the amount of securities backing the note-issue should be £10,700,000 (representing the current level of the permanent Government debt to the Bank) plus such additional quantity as would cover the collateral advances and the deficiency bills.[8] In fact, with that amount the Bank would at all times have been able to issue notes against the total of its permanent, plus its maximum normal temporary, advances to the Government. As, prior to a 25 per cent. refund in 1834, the permanent Government debt corresponded almost exactly with the capital, this computation may have given rise to the idea that the £14,000,000 represented a modernisation of the permission given to the Bank in its original Charter, to issue notes up to the amount of the capital of the Bank, which it had lent to the Government.[9]

A second version, which again we owe to Sir John's researches, is found in the second report of the internal Bank Committee just mentioned. £14,000,000 there appears in the guise of £12,000,000 to be issued against securities, plus £2,000,000 against " unemployable deposits ".[10]

A third explanation, for which we have the authority of Torrens, is as follows:[11]

> ". . . the Bank Notes in circulation in December, 1839 (Bank Post Bills not included) must have been £14,732,000. To this amount, however, must be added the amount of notes, which, had the separation of functions then existed, the Banking department would have been obliged to keep in its till to meet the demands of its depositors. This amount, we know, has been reduced on a recent occasion to £2,000,000 . . . Upon these grounds the framers of the Act assumed that, under a separation of functions, the minimum amount below which the notes out of the issue department could not be reduced under

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[2] Evidence to House of Lords Committee, reprinted in Hayek's edition of *Paper Credit*, p. 297.

[3] QQ. 2222, 2230.

[4] Q. 523.

[5] Q. 3746.

[6] Q. 365. The figure of £10 million as a bullion reserve was independently advocated (e.g., by Tooke), *op. cit.*, III, p. 187.

[7] Hankey, *op. cit.*, p. 8.

[8] Clapham, *op. cit.*, II, p. 174.

[9] 5 W. and M., c. 20, s. 26.

[10] *Op. cit.*, II, p. 175.

[11] *The Principles and Practical Operation of Sir Robert Peel's Bill of* 1844, *explained* . . . (1848), quoted Hankey, *op. cit.*, p. 7.

> the action of the foreign exchanges, was £16,732,000 ; that if £14,000,000 were permitted, under the provisions of the Act, to be issued without a corresponding reserve of bullion, the minimum amount which would be required to be issued against bullion held in deposit would be £2,732,000; and that as the reserve of bullion could not, under such circumstances, be ever reduced below £2,732,000, the convertibility of the circulation would be secured."

Which of these derivations is actually responsible for the figure of £14,000,000 it is now impossible to say, but in all probability it represented the focus of two or three separate lines of argument.

In the form in which the proposal was embodied in the Act, the Bank was allowed to keep its bullion partly in silver, with a top limit of 25 per cent. of the total reserve. This provision appears to have originated with the Government. It was explained by Peel as intended to meet the requirements of merchants dealing with silver standard countries.[12] but the Bank does not appear to have made any appreciable use of the concession. This was just as well, as the coagulation of part of its meagre reserve in a form which was not legal tender might have been somewhat embarrassing in the Bank's more penurious periods.

It is perhaps appropriate at this point to refer to the principal omission from the Act, as compared with previous discussions along similar lines—*viz.*, the failure to provide a Relaxing Power by which the Fiduciary Issue could have been expanded in case of need. Even Loyd had, in 1840, favoured the inclusion of such a provision,[13] though it is true that he also suggested, in the same year, that it was not necessary if direct control was exercised over the Country Banks' issues.[14] Its omission from the Bill, which was to be a main focus of controversy during the next twenty years, was immediately attacked by Palmer in a letter to Peel, written while the Bill was before the House.[15] But Peel, supported by Sir Charles Wood, Chairman of the 1840 Committee, resisted the concession on grounds typical of true politicians, the former asserting that it was better not to commit future Governments,[16] and the latter that it would subject the Government to intolerable pressure.[17]

V

The third main subject of the Act, and the one which occupies three-quarters of its clauses, was the concentration of the note-issue. Most of the controversy here centred avowedly around the desirability of the Bank's monopoly. Earlier in the century, proposals for limiting country bank issues were made in the belief that the Bank of England was unable adequately to limit them.[1] Between 1804 and 1830 (or thereabouts), however, the usual view was that expressed by Thornton[2] and Ricardo,[3] viz., that by one channel or another the Bank of England was able to exercise effective control over the provincial issues. Consequently the controversy during this period was carried on only by those with other axes to grind—for example, Ricardo, in 1816, seeking to protect the public against losses through the failure of mushroom banks[4] and Parnell (1828) using the same argument to support his free-banking plan of substituting a group of strong joint-stock banks.[5]

The 1832 Committee, however, regarded the monopoly question as the most important issue of the day, and made persistent enquiries into the desirability of some limitation of the country banks' issues. Its witnesses were nearly equally divided for and against. So also were those who testified in 1840/41, but by the latter years the arguments used had changed. In 1832, those who gave evidence against the Bank's monopoly were mainly opposed to the restriction on country banks' issues *per se;* by 1840, the objections were chiefly against the Bank of England as monopolist. Some attention was, indeed, devoted by this later Committee to the question whether it might not be better to continue the process by which country banks were paid to circulate Bank of England notes instead of their own, rather than that the power to issue should be withdrawn from them altogether.[6]

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[12] Speech in House of Commons, 20th May, 1844.

[13] *Thoughts on the Separation of the Departments* (1840) in *Tracts*, p. 282.

[14] Evidence before 1840 Committee, Q. 2879.

[15] Quoted Feavearyear, *The Pound Sterling*, p. 256.

[16] " . . . we are taking all the Precautions which Legislation can prudently take against the Recurrence of a Pecuniary Crisis " yet " it may occur in spite of our Precautions ; and if it does and if it be necessary to assume a grave Responsibility for the purpose of meeting it, I dare say Men will be found willing to assume such a Responsibility." (Letter to Cotton, quoted by him to Lords' Committee of 1848, Q. 3284).

[17] *Hansard*, LXXIV, p. 1371.

[1] e.g., *Thoughts on the present Prices of Provisions* . . . (Anon., 1800), p. 26.

[2] *Paper Credit* (Hayek's edition), p. 208.

[3] *Reply to Mr. Bosanquet's Practical Observations* . . ., Chapter VII.

[4] *Economical and Secure Currency* in *Economic Essays* (Gonner's edition), p. 174.

[5] *Observations on Paper Money* . . ., pp. 36–7, etc.

The cross-examination of Norman before this Committee, however, affords a significant clue to the convictions which Peel was already forming. He wound up a lengthy discussion with the witness by saying: "Assuming, for the sake of argument, that it was desirable for the Bank of England to possess this exclusive control over the circulation ; upon that assumption, do you not think that it would be a preferable arrangement to make a provision of this kind, that hereafter no new bank of issue should be formed; that ten years hence existing banks of issue should not be allowed to issue their own notes; that in the meantime it should be competent to the Bank of England, and to the existing banking companies, to make an arrangement in the discretion of each, for the substitution of Bank of England notes for the present notes?"[7] It is clear that in 1840 Peel was prepared to be even more drastic in the curtailing of country bank notes than was, in fact, provided for in his Act. No doubt subsequent reflection, coupled possibly with remonstrances from the Bank of England at the discussion in January, 1844,[8] and possibly with the expectation of opposition from country bankers to any such drastic measures, caused him to feel that he need not go quite so far. In the event he decided instead to allow the natural processes arising from the Act of 1833, and the visible growth of joint-stock banking, to achieve the end he had in view. History records how well his expectations were fulfilled in this, though not in the main purpose of the Act.

It may perhaps be remarked that the provision in Clause 5 of the Act, for the Bank of England to absorb into the Fiduciary Issue two-thirds only of any lapsed country issue, is explained by an implicit supposition that country banks kept a reserve of gold equal to one-third of their note issue, and that since the suppression of the note-issue would release this reserve into circulation, it was unnecessary to replace more than two-thirds of the lapsed paper.[9]

Finally, reference should be made to the provision for the publication of the Bank's accounts. This subject was one of the three topics which the Committee of 1832 conceived themselves chiefly required to investigate. At that time there was a marked cleavage of opinion between different witnesses to the Committee; even the Directors of the Bank were divided, as Norman explained in his evidence.[10] He himself was in favour of publicity, whereas Palmer thought that publication would be undesirable, since the disclosure of the Bank's weakness in times of anxiety might lead to panic.[11] Loyd, however, dogmatic as always, regarded the regular dissemination of information about the Bank's accounts as the "first great essential"[12] and the mass of statistics about the Bank's position which the 1832 Committee itself collected, and for the first time made public, led naturally to the requirement of the Act of 1833 that a weekly return was to be made to the Chancellor of the Exchequer. This return was not to be published, but quarterly averages were to be issued one month in arrear, and these furnished the market with some, if belated, information about changes in the Bank's position.

During the next few years the demand for prompt publication grew in strength[13] and the value of even the available details in analysing the causes of the 1839 crisis probably both reassured the more timorous opponents of publicity and justified the expectations of those who had contended for it. Certainly the 1840 witnesses appear to have taken it almost for granted that fuller publication of the Bank's figures would be forthcoming.[14] The form dictated in the 1844 Act is merely a re-arrangement of the details required in 1833, suitably modified to cover the Separation of the Departments.

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[6] Norman's evidence, *especially* QQ. 1987—2001.

[7] Q. 2125.

[8] Although some consolidation of their monopoly was sought by the Bank's spokesmen, and individual Directors, such as Palmer, aspired to make it complete, the programme presented to Peel by Cotton and Heath suggests that the official view of the Bank was that *restriction* of other issues would suffice.

[9] *Bankers' Magazine*, March, 1866, quoted Gilbart, "The History . . . of Banking" (1893 edition), II, p. 121.

[10] Q. 2694.

[11] Q. 736, etc.

[12] Q. 3382.

[13] See, e.g., Loyd, *Reflections* (1837).

[14] See QQ. 2880—2882 (Loyd); 2061, 2130–3 (Norman).

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Such, then, were the diverse elements from which the Act of 1844 was constructed —the Act which Professor Gregory has rightly described as " an attempted solution of certain definite difficulties, and not a mere application of an *a priori* theory.[15] Of the positive intentions of its designers little remains to-day; but its negative achievements, and in particular the elimination of competing issues, enabled the Bank to avoid further State interference for 70 years, and shaped the development of central banking as we know it to-day. Its authors, surveying their handiwork from the Elysian Fields, may well reflect that they builded better than they knew.

[15] *British Banking Statutes* . . ., I, xvii.

Some Aspects of the Malayan Rubber Slump 1929-1933[1]

By P. T. Bauer

1. There is general agreement among economists on the causes underlying the violent price fluctuations (often in response to small disequilibria) of primary products, and on the effects of these price-swings on the economies of the producing territories. The purpose of this article is to review some figures illustrating the intensity of the great rubber slump of the early 1930s, especially in Malaya, and also to discuss some of its less familiar aspects, such as the sharp fluctuation in agricultural employment in the chief producing territories. An attempt will also be made to show the relative attractiveness of rubber and rice growing to the Malay smallholder during the slump which raises questions of considerable importance for the whole Malayan economy.

2. The particularly severe price fluctuations of rubber have frequently been commented on. Lord Keynes calculated in 1938 that " there has only been one year in the last ten in which the high price of rubber exceeded the low by less than 70 per cent. The average excess of the year's high over the year's low has been 96 per cent. In other words, there is on the average, some date in every year at which the price of rubber is approximately double its price at some other date in that year ". (*Economic Journal*, September, 1938.)

The pride of place occupied by rubber among raw materials subject to violent fluctuations, also emerges from a table prepared by the New Zealand Law and Mercantile Agency Co., Ltd.

TABLE I

VARIATIONS IN RAW MATERIAL PRICES:

	Average Annual Variation 1921/38	*Lowest Price* 1921/38 *as % of Highest Price*
	%	%
Wool 64s tops	24	25
Copper	26	27
Tin	28	31
Cotton	31	17
Rubber	47	3
Butter	23	32
Wheat	30	29

For the purposes of this article the Singapore price of rubber is most directly relevant. Table II summarises its precipitate fall after 1929.

TABLE II

AVERAGE SPOT PRICE OF RUBBER (RIBBED SMOKED SHEET) IN SINGAPORE, 1929–33
(Straits cents. lb.)

1929	1930	1931	1932	1933
34.48	19.31	9.96	7.01	10.23

In June, 1932, the price touched 4¾ cents.

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[1] Extracted from material collected by the author as Leon Fellow of the University of London.

When considering the price fall after 1929 it must be remembered that the rubber industry was not particularly prosperous in that year. The price of rubber was not as unsatisfactory as was made out at the time by many producers, especially the former supporters of the Stevenson Scheme, yet virtually every Malayan Administration Report for the year which mentioned rubber referred to its low, depressed or even distressing price.[1]

3. For a full appreciation of the dependence of the Malayan economy on rubber and the part played by it in the collapse after 1929 it would be necessary to have a reasonably accurate estimate of Malaya's national income throughout this period. This cannot be obtained. A very rough calculation[2] of the gross value of agricultural and mining output (manufacturing industry being in its infancy) shows that the former totalled 430±20 million Straits dollars in 1929 and 130±15 million dollars in 1932, while the value of mining output was around 138 millions and 46 millions respectively. Rubber contributed 344 and 66 millions and tin 122 and 40 millions to the total values in the two years.

4. These figures illustrate one aspect of the importance of rubber in the Malayan economy. The same point can also be seen from a different angle in the Malayan trade returns which also well reflect the impact of the depression. Moreover import figures will be found useful as a consumption index over an unusually wide field. The complications introduced by the division of Malaya into Straits Settlements, Federated Malay States and Unfederated Malay States (S.S., F.M.S. and U.M.S.) must be remembered. Net export and import figures are available for the F.M.S. and the most important U.M.S., but not for Malaya as a whole, since the authorities considered it impracticable to compile net figures in view of the importance of the entrepot trade of Singapore and Penang. It will be shown, however, that the trend of net export and import figures for Malaya as a whole did not differ greatly from that of the F.M.S.

TABLE III

NET EXPORTS OF F.M.S. PRODUCE, 1929/33—MILLION STRAITS DOLLARS:

	1929	1930	1931	1932	1933
Total	338	202	118	83	108
Rubber and Latex ..	202	108	54	37	58
Tin and Tin Ore ..	117	77	51	31	37

These figures include parcel post and bullion, between them less than one per cent. of total net exports. Rubber and Latex was almost entirely rubber, and tin and tin ore almost entirely tin ore. Thus, in 1929 rubber and tin accounted for 94 per cent. of F.M.S. net exports with rubber just under 60 per cent. of the total. More than half of the balance was made up by coconut products.

The decline in the net imports of the F.M.S. was also very steep, from 190 million dollars in 1929 to 63 millions in 1933. In this context it should be noted that the F.M.S. (and indeed Malaya as a whole) were among the most open systems in the world. They depended on imports, not only for all their manufactured goods, even the simplest, but for oil, petrol, sugar, tea, tin, rice (two-thirds of the F.M.S. rice consumption is supplied by imports), tinned milk (the output of fresh milk is very small), edible oils and fats, most of their meat, a host of other foodstuffs and virtually all their tobacco. For a number of highly important articles, net imports provide a reliable consumption index. This did not apply to coconuts, which were produced locally in abundance, to rice and vegetables, to pig's meat, poultry and eggs; of these there was an appreciable local production, even though it was insufficient to cover requirements. The following figures of net imports during these years may be of interest:—

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[1] E.g., the Annual Report for 1929 of the Chief Secretary to the F.M.S. Government and the Annual Reports for the same year of the Agent in Malaya of the Government of India, of the British Residents in each of the F.M.S., of the Commissioner of Police, F.M.S. and many other official documents reviewing 1929. *The Economist* stated early in 1930 that but for their successful forward sales, very few rubber companies would have paid a dividend for 1929.

[2] The basis of the calculation, its component elements and limitations cannot be set out here. It is hoped to publish them eventually as part of a study on the plantation rubber industry.

TABLE IV

F.M.S. NET IMPORTS 1929–32

	1929	1930	1931	1932
Tinned milk: cases ..	467,203	449,139	342,440	214,994
Tea: lb.	4,408,079	3,801,147	2,969,202	1,852,182
Tobacco, Cigars, Cigarettes: lb.	10,718,386	8,657,167	6,416,819	3,277,576
Matches: per 10,000 ..	565,861	389,883	248,841	179,498
Beer and Ale: Gallons ..	297,509	233,544	147,993	114,551
Arrack and Samsoo[1]: Gallons	99,894	55,903	20,557	7,009
Cotton Piece Goods: Yards	30,012,452	24,969,291	19,842,737	18,106,948
Kerosene Oil: Tons ..	31,456	20,993	15,532	13,822
Motor Spirit: Tons ..	46,617	47,683	33,485	22,545
Cycles	6,065	1,990	709	249
Motor Cars	2,536	1,441	491	175

These figures indicate a severe reduction in consumption by all classes and races and present an instructive contrast with the experience of Britain, where real consumption was well maintained during the slump.[2]

As was said earlier, gross figures only are available for the foreign trade of Malaya as a whole.

TABLE V

GROSS MERCHANDISE IMPORTS AND EXPORTS OF MALAYA 1929/33: MILLIONS OF STRAITS DOLLARS:

	1929	1930	1931	1932	1933
Imports	861	706	453	377	350
Exports	925	658	401	323	373

It is possible to estimate roughly the share of rubber and tin in the exports of Malayan produce. In his Report on the Foreign Trade of Malaya in 1929, the Director of Statistics, S.S. and F.M.S. gave a list of all the important exports of Malayan produce (though without stating their values) and added that the exports of all other commodities were re-exports. From that list in conjunction with the 1929 Malayan exports it emerges that rubber and tin accounted for not less than 85 per cent. and probably nearer 90 per cent. of Malayan domestic exports in that year. The exact percentage cannot be ascertained as there was also an important entrepot trade in the raw materials which comprised Malayan domestic exports and no sufficiently comprehensive stock figures are available for an accurate year to year calculation. The estimate of 85 per cent. is, however, definitely conservative for 1929. Rubber was some 58–60 per cent. of the total.

While net imports are not available gross imports minus gross exports do not serve as a good consumption index for Malaya as a whole because of changes in stocks and differences in quality. It is, however, worth stating as a broad indication of the general trend of consumption that the figures of gross imports minus gross exports of the most important consumption goods, though showing erratic variations from year to year, moved over the whole period in a way similar to the net imports of the F.M.S.

[1] These are rice spirits. The local distillation of samsoo exceeded imports. It fell from 245,000 gallons in 1929 to 35,000 in 1932. The number of gallons of liquor of all descriptions (except toddy) imported into and manufactured in the F.M.S. fell from 1,667,596 in 1929 to 319,525 in 1932. In general it appears that net imports serve as a useful index of the trend of consumption in the few instances where they can not be accepted as a complete consumption index.

[2] The decline in consumption was the combined result of a sharp fall in consumption per head and to a lesser extent of the mass emigration of Chinese and Indian labour.

5. Public finance figures are also useful as an index, albeit only a partial one, of the deterioration in economic conditions. For fiscal purposes the Straits Settlements, the Federated Malay States, and the various Unfederated Malay States are entirely separate administrations. The budget figures of the S.S. cannot be used for precise year-to-year comparisons for a variety of reasons, such as frequent transfers between the general revenue and the funds of the Currency Commissioners, and because of administrative changes in the opium monopoly. Broadly, however, the trend of budgetary receipts was similar to that in the F.M.S., though the fall was less steep. The same applied to the various U.M.S.

The F.M.S. budget figures include Federal, State and local expenditure and receipts. Gross receipts and expenditure of public utilities, and the net working results of the F.M.S. Railways are also included. The figures were taken from the F.M.S. Estimates and the Annual Reports of the Auditor-General and of the Chief Secretary to the F.M.S. Government. The following table summarises the budgetary results for the years 1929/33 :—

TABLE VI

F.M.S. GOVERNMENT REVENUE AND EXPENDITURE 1929/33. IN MILLION STRAITS DOLLARS:

	1929	1930	1931	1932	1933
Revenue	82	66	52	44	47
(Estimated Revenue) ..	(70)	(83)	(71)	(56)	(44)
Expenditure	85	82	62	54	50

Revenue estimates were upset with startling rapidity. The 1932 F.M.S. estimates were passed in November, 1931, with a revenue estimate of 56 millions. Three months later a Governmental circular revised this to 47 millions and in a few weeks' time this was again reduced to 42 millions. The steep fall in revenue was regarded with great anxiety by the authorities and by public opinion and within the general structure of the existing taxation great efforts were made to improve the financial situation. The existing indirect taxes were raised sharply and the range of indirect taxation extended enormously. Before the slump, the customs tariff of the F.M.S. was very short (petrol, tobacco and matches, liquors and spirits only). Between the end of 1930 and 1932 the rates were raised several times and taxation extended to sugar, all edible oils and fats, tinned milk, coffee, tea, fruits and vegetables, kerosene oil and textiles (including eventually a specific import duty on the cheapest textiles at a rate not far short of 100 per cent. *ad valorem*), while a host of manufactured articles also became liable to duty. Whatever its merits in equity, this system proved ineffective to improve the financial situation owing to the steep fall in general consumption.

6. The economic crisis was all-pervasive. Postal traffic contracted sharply and the turnover of the most important sections of the F.M.S. Postal Department fell by 50 per cent. or more; the number of letters and telegrams handled all showed decreases of that order.

There was a spectacular rise in crime throughout Malaya. The Commissioner of the F.M.S. Police regularly opened his Annual Report with a review of the Singapore rubber and tin prices during the year, to prepare the reader for what to expect. In the F.M.S. the number of cases of murder, robbery and gang-robbery rose from 160 in 1929 (itself the highest year since 1924, which was attributed by the Police Commissioner to the rubber and tin slump in 1929) to 358 in 1930, 352 in 1931 and 308 in 1932. Cases of gang-robbery rose from 21 in 1929 to 90 in 1930, 108 in 1931, and 91 in 1932. There was a steep rise in house-breaking throughout Malaya. In Johore the figures rose from 138 in 1929 to 373 in 1932; in the F.M.S. there was no separate classification. All the administration reports agreed that the sharp rise was due to the effect of the slump. The F.M.S. Police Report for 1931 stated: " that the prevailing economic conditions should be reflected in crime figures is only natural and accepted. Reports of seizable offences totalled 10,584 compared with 9,052 in 1930, an increase of 16.9 per cent., while compared with 1929 the increase is 29.4 per cent.". The 1932 Report contained this depressing passage: " A considerable proportion of robberies and gang-robberies was the work of men searching for food, rather than of organised robber-gangs, while the increase in thefts and house-breakings was undoubtedly due to the need for food ". Quite unexpected forms of crime appeared or re-appeared. " Piracies became so numerous among the fishing fleets operating between the Selangor coast and Sumatra that special measures had to be taken ". (Annual Report for 1930 of the British Resident of Selangor.) The rise in criminality is more impressive when it is remembered that several hundred thousand Indian and Chinese coolies left Malaya between the middle of 1930 and the end of 1932.

7. The income and price deflation was so complete that prices usually considered particularly stable or " sticky " shared in the general collapse. The Municipal Assessor of Singapore published an index of assessment value of 489 houses, comprising all ranges of rent values, and based on the rents of the preceding year. It fell from 115 in 1929 to 65 in 1934 ; similar declines took place in Penang and Kuala Lumpur. According to the Commissioner of Lands, S.S., rural land values had declined by 70 per cent. between the end of 1929 and the end of 1932.[1]

Much of the income and price deflation was borne by property owners and by non-resident shareholders of rubber and tin companies; nevertheless, its effect on wage-rates and workers' earnings was spectacular. Daily wages of South Indian estate labourers fell by some 60 per cent. between the end of 1929 and mid-1932, from some 50–55 Straits cents a day to around 20 cents. The 1929 rates were among the highest, probably the highest, daily rates received by agricultural labourers anywhere in South or South-east Asia. Moreover, in 1929 there was no involuntary employment among the workers' dependants, while in 1931/32 work at 50/60 per cent. below 1929 rates was available only on four or five days a week, with no employment for dependants, so that earnings, especially family earnings, fell more steeply than wage rates. In the late Spring of 1932, when the slump was at its worst, Indian tappers' wages around, or even below 20 cents, were some 25–30 per cent. lower than they had been in 1914. While in 1929 the Tamil coolie in Malaya was definitely prosperous by Asiatic standards, his wages in 1932 were near starvation rates and were totally inadequate for the maintenance of dependants.

Wages of Chinese estate labourers, employed chiefly on Chinese-owned properties or by contractors carrying out special works such as felling, clearing or bud-grafting, fell proportionately much more. At the end of 1929 their daily earnings were around 80–90 cents; by mid-1932 they were about level with Indian wages. Chinese coolies and their employers frequently have a definite profit-sharing outlook on wage-earning and this was one, though not the only reason for specially violent fluctuations in the wages and earnings of Chinese coolies in Malaya. In many instances Chinese coolies in 1931–32 worked for board and lodging and perhaps 5–10 cents a day.

8. The steep fall in estate wages would suggest that estate employment, at any rate, was reasonably stable. This was by no means so, and its very sharp fall in Malaya and the N.E.I was a somewhat unexpected feature of the slump.

Employers of ten or more Indian labourers in Malaya were legally bound to furnish quarterly returns of the number of labourers in their employ. This covered all estates of any size. Figures of the employed South Indian estate population are available throughout the slump for the F.M.S. and the Straits Settlements, but not for the Unfederated Malay States, where they were published only from 1933 onwards. The F.M.S. and the Straits Settlements comprised some 75 per cent. of the Indian estate population of Malaya, and estate employment was affected by much the same factors throughout Malaya; this was fully confirmed after 1933, when estate employment in the Unfederated States was also published. It may be mentioned as a point of interest that the plantation industry (in practice, the rubber industry) was the only important private employer of Indian wage-earning labour among employers with ten workers and over; at the end of 1929 it accounted for some 97 per cent. of privately employed Indian wage-earners in the F.M.S. (i.e., excluding Indians employed by Government Departments), and for some 85 per cent. in the S.S.

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[1] The fall in the value of poor yielding, marginal, land, especially rubber estates, was much more severe, and indeed on occasions, so great as to be scarcely believable. In the Autumn of 1931 a low-yielding rubber estate on Singapore Island was sold for the equivalent of 3/– per acre ; in 1929 £15 and in 1926 £40 per acre had been refused for the same property.

TABLE VII

SOUTH INDIAN ESTATE EMPLOYMENT IN THE S.S. AND F.M.S., 1929/33. NUMBER OF SOUTH INDIAN LABOURERS EMPLOYED ON ESTATES AT THE END OF EACH YEAR; FIGURES TO THE NEAREST 1,000

1929	1930	1931	1932	1933
206,000	154,000	120,000	104,000	111,000

Source: Annual Reports of the Controller of Labour S.S. and F.M.S.

Indians were by far the most important source of estate labour, especially on European-owned estates. Chinese employment is numerically less important and the data relating to it are both more complex and far less reliable. Its inclusion would have complicated the discussion, without affecting the conclusion.

The fall in employment, taken in conjunction with that in wage-rates, suggests a decline in pay-rolls of about 80 per cent. between the end of 1929 and the middle of 1932.

The fall in employment offered by the highly-diversified tropical agriculture of the Netherlands East Indies was proportionately much the same as in Malaya. Figures of estate employment are available for the Outer Provinces only (i.e., excluding Java and Madura). Employment figures on estates in the Residency of the East Coast of Sumatra, the most important planting district of the Outer Provinces, are given separately.

TABLE VIII

NUMBER OF EMPLOYED ESTATE COOLIES (TO THE NEAREST 1,000) ON ESTATES IN THE OUTER PROVINCES OF THE NETHERLANDS EAST INDIES AND OF THE EAST COAST OF SUMATRA, 1929–33. END OF YEAR.

	1929	1930	1931	1932	1933
Outer Provinces . .	521,000	479,000	360,000	263,000	243,000
East Coast of Sumatra . .	320,000	294,000	238,000	176,000	186,000

Source: Statistical Abstract of Netherlands India.

It would appear that under certain conditions fluctuations in agricultural employment can be quite as violent as, or even more so than, those of employment in manufacturing industry.

9. The widely-held view that the supply-curve of Malay native rubber was backward-rising was refuted by the experience of the slump. Whether expressed in terms of total production, or as output per mature acre, the supply-curve, though very inelastic, was on balance forward-rising.[1] The following table should be read in conjunction with the prices given in Table II, though an allowance must, of course, be made for the margin between Singapore and up-country prices.

TABLE IX

TOTAL PRODUCTION AND OUTPUT PER MATURE ACRE OF MALAYAN NATIVE RUBBER 1929—1933

	1929	1930	1931	1932	1933
Total Production (tons) to the nearest 1,000 tons	200,000	197,000	197,000	177,000	220,000
Output per mature acre (lb.) to the nearest 5-lb.	485	460	445	385	465

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[1] Output per mature acre frequently expresses the rate of capacity working much better than does the total production. During the slump years the total area in bearing increased considerably (especially outside Malaya and Ceylon), as a result of the planting boom of the mid 1920s, and total production figures tend to give a misleading idea of the reaction to the price fall of several important groups of producer. In terms of capacity working the supply of rubber by estate producers in Java, native producers in the Outer Provinces of the N.E.I., and of practically all producers in the minor producing territories was much more elastic than would appear from output figures.

The production figures are official, but actually represent sales by smallholders to dealers; the output per mature acre is based on the records of the International Rubber Regulation Committee, and only indicate the general order of magnitude.

Quarterly figures show the forward-rising tendency somewhat more markedly.

10. The high level of Malay native rubber production during the slump was much commented on at the time and generally explained in terms of the natives' need for cash. This seems a somewhat misleading way of stating the matter. It appears that practically throughout the depression rubber was a more attractive product than rice for the average smallholder of Western or Southern Malaya who could obtain more rice (the price of which had also fallen steeply) by producing rubber, selling it and buying rice with the proceeds than by growing the latter direct. In view of the intensity of the rubber slump this proposition might be received with some surprise but there is substantial evidence to support it. In Tables X and XI rubber and padi (the unhusked grain of the rice plant) are compared, first as means to secure a given quantity of rice and secondly as cash crops.[1] The sources, assumptions and calculations underlying the data are reviewed in the Appendix.

TABLE X

	(1) Average yield of small-holders' rubber lb. per mature acre	(2) Singapore price of rubber smoked sheets cents lb.	(3) Assumed average price received by small-holders; cents lb.	(4) Estimated gross proceeds acre dollars	(5) Assumed expenses per acre dollars	(6) Estimated net proceeds per acre dollars
1929	485	34.5	30.5	150	6	144
1930	460	19.3	16.8	77	6	71
1931	445	10.0	8.0	36	3	33
1932	385	7.0	6.0	23	3	20
1933	465	10.2	8.7	40	3	37

	(7) Average retail price of No. 1 Rangoon rice in Malacca cents gantang (1 gantang rice = 8 lb.)	(8) Gantangs rice obtainable with proceeds of rubber (6)÷(7)	(9) Average yield of cleaned rice gantangs per acre	(10) Deduct rice equivalent of expenses gantangs	(11) Net yield of rice (9)−(10)	(12) Balance in favour of rubber in gantangs of rice (8)−(11)
1929	52	277	83	30	53	224
1930	46	154	73	30	43	111
1931	28	118	101	30	71	47
1932	22	91	110	30	80	11
1933	23	160	106	30	76	84

TABLE XI

	(1) Average yield of padi per acre gantangs	(2) Price per gantang cents	(3) Gross Proceeds from padi dollars per acre	(4) Cash equivalent of cost padi production dollars	(5) Assumed net proceeds from padi dollars	(6) Estimated net proceeds of rubber as Table X (col. 6)	(7) Difference in favour of rubber dollars
1929	202	14	28	14	14	144	130
1930	180	13	23	13	10	71	61
1931	248	8	20	7	13	33	20
1932	272	7.5	20	6	14	20	6
1933	260	6.7	17	6	11	37	26

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[1] By far the greater part of the Malayan rice output is consumed by the growers and padi is not usually marketed; accordingly, the comparison attempted in Table X is of greater significance and also less hazardous than that in Table XI.

On the assumptions of the Tables Malay smallholders not too far from the principal rubber markets could, even in 1932, obtain more rice indirectly (i.e., by purchasing it with the proceeds of rubber growing) than by producing it direct, a somewhat unexpected state of affairs in the worst year of the acutest rubber slump of all time. The relationship was probably reversed for a few months in the Spring of 1932 when rubber was around 5 cents in Singapore. The figures are rough averages and conditions differed in various parts of the country but the conclusions would be valid for most of Western and Southern Malaya where smallholders were particularly dependent on rubber.

11. It would thus appear that the preference of many smallholders for rubber was neither irrational nor enforced by desperate need for ready cash. It seems rather to have rested on the realisation that in relation to available alternatives rubber remained the most attractive crop. Evidence of the rational conduct of the smallholders was also furnished by the estimates of the Malayan Department of Agriculture of the areas out of tapping on smallholdings in 1932 and 1933; the largest percentage of the mature area out of tapping was invariably on the poor-yielding areas of the Straits Settlements, especially on Singapore and Penang Islands, and to a lesser extent in remote districts like parts of Pahang, which were furthest from the markets of Singapore and Penang. The area out of tapping reacted promptly to price changes.

Reference to prices and proceeds actually understates the advantages of rubber over rice. The former, indeed, in spite of its violent price fluctuations, appears particularly well suited to the needs of the Asiatic smallholder. It is non-seasonal and thus the problem of maintaining the cultivator and his family until the harvest, a prime cause of the notorious indebtedness of the smallholder, does not arise. Again, the various items of expenditure in rice cultivation, such as the purchase of manures, of a plough, or of a draught animal, are all likely to send the smallholder to the moneylender-cum-shopkeeper. The weather risks, negligible in rubber production, are considerable in padi growing. Moreover, even during the Spring of 1932, rubber was always easily saleable, and the ubiquitous Chinese dealers seemed to have been sufficiently numerous in all rubber-growing districts to ensure a fairly lively competition among buyers. This was by no means generally true for padi, either in Malaya or elsewhere in the East. There is another important consideration which should perhaps have been put first. Rubber growing entails much less hard work than padi-culture. Instead of toiling in the mud of a wet padi field, sometimes for days on end, the rubber growing Malay native never had to work more than three or four hours a day, and could take off a day or even a week whenever he felt like it; rubber tapping is generally believed to be the least exacting form of work in tropical agriculture.

12. During the depression, planters and officials, interested and disinterested observers alike, deplored the heavy native rubber production, and the dependence of the smallholder and of Malay as a whole on rubber. Actually, the Malay population was not so dependent on rubber as had been believed, and the Malayan Rice Cultivation Committee of 1930, estimated that the Malays grew some 75–80 per cent. of their total rice requirements; the heavy rice imports were needed primarily to satisfy the needs of the Indian and Chinese population. In certain districts of Western and Southern Malaya, the Malays depended to a fairly large extent on rubber, but were apparently fully justified; moreover, it also seemed that they had not lost the art of padi growing, and were quite capable of returning to it when the economic situation so demanded.

These considerations are relevant to important issues of policy. The prosperity engendered by the rubber industry had raised the standards of living of all classes in Malaya (including the natives), far beyond what could have been hoped for from her naturally poor soil. Contrary to popular belief, Malaya is by no means a fertile country, much less so, for instance, than Java, where the standard of living was lower than in Malaya. During the slump, various steps, including the imposition of an import duty on rice, were taken to diversify the Malayan economy and to reduce the dependence on imported food. It is not possible in the concluding paragraphs of a short article to discuss adequately the merits of such a policy, or to review the possible alternatives. It may be suggested briefly and tentatively that the maintenance by the Malayan Governments of a large revolving stock of rice would seem a better policy of economic and perhaps also of military security. With an unfavourable rubber/rice price ratio[1] the cultivation of the latter (both as dry and wet padi) could be increased sufficiently to render the Malay population self-sufficient within a few years.[2] Should the deterioration be permanent, and an indication of a long-period decline of the rubber industry, a large-scale repatriation of Indian and Chinese labour would have to be accompanied by the expansion of food production by and for those who remain.

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[1] As long as the ratio is in favour of rubber production it will not be easy to persuade the smallholders to abandon rubber in favour of padi. It should be stated, however, that largely as a result of the Malayan Drainage and Irrigation Departments created in 1932, there was an improvement in Malayan padi yields during the 1930's. A further increase may necessitate a partial reconsideration of some of the conclusions above.

[2] Padi growing by non-Malays raises political issues outside the scope of this article.

APPENDIX

The following notes summarise the sources of the data for Tables X and XI.

The spread between the Singapore price and that received by the Malayan smallholder for the bulk of his rubber was generally narrow, especially at times of low prices. The margin may be taken as 4 cents. for 1929 (a year of comparatively good prices), 2½ cents for 1930, 2 cents for 1931, 1 cent for 1932 and 1½ cents for 1933. No accuracy is claimed for these figures, but they show the general order of magnitude ; they are in accordance with the observations of the late Dr. Whitford in his various " Reports on Plantation Rubber " and are also in rough agreement with the prices published in the market reports of the Malayan Department of Agriculture. With the latter, the comparison is not easy as in the market reports a range of prices used to be set against a Singapore average. The figure of one cent as the average margin for 1932 was also confirmed by some remarks of an Unofficial Member in the F.M.S. Federal Council in the Autumn of 1933. The keen buying competition of European and Chinese dealers and the proximity of the most important native rubber growing districts to the larger towns explains the remarkably low margins. The bulk of Malay native rubber was well prepared and generally good quality. In Singapore Chinese Smoked Sheet (Malayan smallholders' rubber smoked by Chinese dealers) fetched much the same price as standard quality ribbed smoked sheet.

The price of padi for 1929, 1932 and 1933 is the average price paid for Malayan padi by the Government mill at Bagan Serai in Perak. The corresponding figures for 1930 and 1931 could not be traced but it is reasonable to assume that the proportionate change in the average price from 1929 to 1930 and 1931 was roughly of the same order as the changes in the unit value of padi in the trade returns : this was true for 1929 and 1932 and 1933. The 1930 and 1931 price has been calculated on that assumption. The price paid by the Government mill was almost certainly higher than the average price obtainable for Malayan padi in the interior and to this extent the comparison is weighted in favour of padi.

The retail price of rice in Malacca was taken from the official annual " Malaya Average Prices " ; for inland centres like Kuala Lumpur only occasional figures were available which were not greatly different from the prices in Malacca. The average yield of padi and rice can be calculated from the official estimates of output and acreage. For 1929 and 1930 the official estimate of rice yields has been raised by ten per cent. as subsequent information revealed that the official figures under-estimated the yields in those years.

Expenditure on production is a more difficult matter, especially as most items of the cost of rice production were paid for in kind and often varied with the yield. The figure of 30 gantangs (240 lb.) of cleaned rice per acre as the rice equivalent of the cost of production is certain to be conservative and below the general average. It is substantially below the estimate of the Malayan Rice Cultivation Committee of 1930. The figure has been adopted after correspondence with Dr. H. A. Tempany, Agricultural Adviser to the Secretary of State for Colonies, who was Director of Agriculture to Malaya throughout the slump. In comparing rubber and padi as cash crops it was necessary to express in cash the costs of padi production. The figure of 30 gantangs was multiplied by the Malacca price of rice (Col. 7 of Table X) and one dollar deducted to allow for the absence of milling when the crop is sold as padi. The conversion of the expenses in kind into cash costs at a retail price undoubtedly inflates the figure ; on the other hand 30 gantangs is more likely to be a minimum rather than an average while the estimated price of padi is also generous so that the net figure is unlikely to be seriously affected.

The expenses of rice production whether in cash or kind were appreciable higher than the cost of rubber growing on small holdings as rubber requires no plough, draught animal, manures or seeds. In 1932 the Kuala Lumpur correspondent of the *Straits Times* put the all-in cost of smallholders' rubber in Selangor at ½ cent per lb. It was not clear from the context whether or not this included rent. The majority of smallholders paid rent at one dollar an acre when growing rubber ; rice land was generally rent free except for irrigation charges and the like. To be on the safe side it has been assumed that the cash cost production was ½ cent per lb. in 1931–33 excluding rent. For 1929-30 one cent per lb. (again excluding rent) has been taken to allow for the higher cost of coagulants and tools in those years. Once again the figures are broadly in accordance with Dr. Whitford's estimates for the years in question. Both in the cost of production and in the proceeds assumed allowance has been made in a rough way for the fact that most of the output was sold in the form of unsmoked sheet.

The Ulster Linen Industry

by A. BEACHAM

IT is remarkable that so very few references are found in current economic literature to the Ulster Linen Industry in view of its importance not only in the economy of Northern Ireland but also in the economy of the United Kingdom as a whole. It has been estimated that directly, or indirectly through subsidiary trades, the industry normally employs nearly one-fifth of the gainfully occupied population of Northern Ireland.[1] The estimated value (free from duplication) of linen goods produced by the industry in 1935 was £9 million of which about 60 per cent. was exported.[2] The importance of the industry to the United Kingdom as a dollar earner is shown by the fact that, in the years immediately preceding the outbreak of the present war, it exported annually to the U.S.A. goods to the value of £1½–£2 million. It seems worth while therefore to attempt a brief description of the pre-war size, scope, and structure of this important but little known industry and also of the manner and extent to which it has been affected by wartime developments.

Before proceeding further it is necessary to define what is meant by the Ulster Linen Industry in this article.[3] It is here defined to comprise all firms engaged mainly or wholly in the spinning of flax, the manufacture of linen thread, the weaving and finishing of linen and union piece goods, and the making up of handkerchiefs, household articles of linen, etc. All of these firms are not engaged wholly in the manufacture of linen. Flax spinning firms, for example, produce a small quantity of hemp and ramie yarns and linen weaving firms normally produce a small quantity of cotton piece goods. The amounts of these products however, are insignificant when compared with the total output of flax yarns and linen or union piece goods. The position is quite different, however, in the finishing and making-up sections of the industry. In 1935 about 20 per cent. of the work done in the making-up section was done on cotton goods and about one-third of the work done by the finishing section was done on cotton yarn and cotton piece goods. Although we are only concerned here with these firms in so far as they work on linen, these facts should be borne in mind when employment figures are quoted.

Although linen firms are to be found in each of the six counties of Northern Ireland, the industry is mainly concentrated within a radius of 30 miles of Belfast. About 50 per cent. of the industry, by value of output, is within the County Borough of Belfast and less than 5 per cent. in the counties of Tyrone, Londonderry, and Fermanagh. In view of its dependence on imported raw materials, and on outside markets for the disposal of its products, it is hardly surprising that the industry is so much concentrated in the vicinity of the only important port in Northern Ireland.

In 1912 it is estimated that the numbers of spindles and looms at work in the linen industry throughout Ireland were 946,000 and 37,000, respectively. (Of these about 3,000 looms and 20,000 spindles were in what has since become Eire.) Since 1912 the numbers of spindles and looms at work in Northern Ireland have declined to about 870,000 and 28,000, respectively in 1937.[4] In 1912 the total output of flax yarns was 32,650 tons and the production of piece goods approximately 222 million square yards[5] for the whole of Ireland. For the area which has since become Northern Ireland, output in 1912 may be roughly estimated at 32,000 tons of yarn and 204 million square yards of piece goods. By 1937 the output of flax yarn had fallen to 31,350 tons and the output of piece goods to 161.5 million square yards.

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[1] *Ulster Year Book*, 1938, p. 81.

[2] *Northern Ireland Census of Production*, 1935, p. 33. This year was in most respects a normal pre-war year for the linen industry.

[3] We shall be mainly concerned in what follows with the main sections of the industry—spinning and weaving—which employ over 70 per cent. of the total personnel engaged in the industry.

[4] A further decline to 840,000 spindles and 26,000 looms was recorded in 1938. Figures for the number of looms and spindles have been taken from the *Belfast and Ulster Trades Directory*.

[5] The recorded figure (1912 Census of Production) was 227 million linear yards. This has been converted into square yards by assuming the same relationship between linear and square yards in aggregate output as obtained in 1924.

Between 1912 and 1937 therefore the number of spindles in place in Northern Ireland declined by about 6 per cent. and the output of yarn by about 2 per cent. Over the same period the number of looms declined by about 18 per cent. and the output of piece goods by approximately 20 per cent.[1] The average number of operatives employed in all sections of the industry declined by about 20 per cent. during this period, the recorded figure in 1937 being 61,900.

Table I shows the volume of production and employment in the various sections of the industry for 1935—the latest year for which detailed figures are available. These figures are believed to give a reasonably accurate impression of the normal volume of activity over the period 1933–38.[2] Both the spinning and weaving sections appear to have been working well below capacity at this time, since in 1932 the employers had estimated[3] that the spinning section was capable of employing 30,000 workpeople and the weaving section 25,000 workpeople, whereas the actual employment figures in 1935 were 24,800 for the spinning section (including linen thread manufacture) and 20,000 for the weaving section.

Table I also shows the volume of linen and union piece goods handled by the finishing section of the industry. Accurate comparisons with previous years are not possible but it is clear that the scope of this section whether measured by volume of employment or volume of goods handled has expanded slightly between 1912 and 1935. The volume of linen and union piece goods handled declined by about 9 per cent. but this was more than compensated for by increases in the volume of linen yarn, cotton yarn, and cotton piece goods handled by the finishing trade.[4]

In 1935 about 44 per cent. of the output of linen and union piece goods (loom state) and about 53 per cent. of the total output of finished piece goods were handled by the Ulster making-up trade for conversion into handkerchiefs, household articles of linen, etc. Roughly then we may say that of the total output of the weaving section about 16 per cent. is disposed of by the industry in the loom state, about 40 per cent. is finished only, and about 44 per cent. is finished and made-up inside the Province.[5] Many linen making-up firms are also extensively engaged in making-up cotton piece goods and there is also a fairly extensive cotton making-up trade in Northern Ireland. It is not possible to measure the extent to which activity in the linen making-up trade has varied during the inter-war years though it is certain that the volume of linen piece goods handled has substantially declined. The extent to which this may have been compensated by increased activity on cotton piece goods cannot be ascertained.

The position of cotton in relation to the Ulster linen industry is worth noting. Cotton spinning, and cotton weaving outside the linen industry, are practically unknown in Northern Ireland, only two very small firms being reported as so engaged in the 1930 and 1935 Censuses of Production. Flax spinning firms spin no cotton yarn but linen weaving firms working on imported yarns weave a small quantity of cotton piece goods—between three and four million square yards per annum. Since imports of cotton yarn into Northern Ireland[1] have not varied appreciably in 1913—1939 (between three and four thousand tons per annum) it is not likely that the output of cotton piece goods or of linen and cotton union piece goods has varied to any great extent during the inter-war years, though such goods formed a higher proportion of the total output of piece goods produced in 1937 than in 1913. Substantial quantities of cotton piece goods, however, are imported into Northern Ireland for finishing and/or making-up.

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[1] These figures suggest that the impact of depressed trade conditions between the two wars has been felt much more keenly by the weaving section than by the spinning section. This impression is apparently confirmed when reference is made to the available output figures for the inter-war years (see Table II). The average output of yarn in the years 1933, 1934, 1935, and 1937 was only 7 per cent. below the estimated 1912 level of production in N. Ireland and 6 per cent. above the 1924 level. The average production of piece goods in the years 1933, 1934, 1935, and 1937 was 26 per cent. below the estimated 1912 level of production in N. Ireland and 5½ per cent. below the 1924 level. Most observers in the trade, however, are agreed that both sections of the trade have been affected to very much the same extent by the depression of the inter-war years. A large part of the explanation of the contrary impression given by the figures in the tables turns round the fact that yarn output is measured by weight and piece goods output is measured by square yardage. It is generally agreed that since 1912 the average quality of yarn produced has become much coarser, i.e., a given length of yarn has become much heavier.

[2] The output of flax yarn, linen thread, and linen and union piece goods in 1935 was within 4 per cent. of the average for the years 1933, 1934, 1935, and 1937, in each case.

[3] Application of the Linen Industry to the Import Duties Advisory Committee. The employers estimated the productive capacity of the weaving section at 240 million square yards. The explanation of this amazing figure appears to be that the square yardage produced per loom is very much dependent upon the type of goods being woven and that there has been a considerable proportionate increase in the production of those types favouring a high productive capacity per loom. A similar but less pronounced change in the weight of yarn produced per spindle seems also to have taken place as a result of the growing average coarseness of yarns produced. These changes have also been partly due to increased efficiency of labour and management. Very little, if anything, is to be attributed to purely technological changes.

[4] Due mainly to loss of trade in plain bleached linens and increased trade in household goods (towels, etc.) made from bleached yarns.

[5] Only a very small quantity of linen piece goods was handled by the Northern Ireland cotton making-up trade in 1935.

The industry relies mainly on imports from the Low Countries and the Baltic for its basic raw material—flax fibre. In 1913 the Irish flax crop was about 14,000 tons[2] and an additional 46,000 tons of flax (including tow) were imported. Since this time the production of flax in Ulster has declined substantially though it has shown considerable variation from year to year. The average annual production of flax fibre in Northern Ireland between 1933 and 1937 was only 4,400 tons—the average annual import of flax (including tow) into the Province over the same period being approximately 37,000 tons. Only relatively small quantities of flax fibre are sent out of Northern Ireland—about 3,000 tons in 1913 and quantities varying between one and two thousand tons in each of the years 1933–37. Only a small part of the total annual import was in the form of tow before 1914 but during the inter-war years substantial quantities of cheap Russian tow have been imported.[3]

Not all of the flax which is spun is also woven in Northern Ireland. On the average about 4,000 tons of yarn per annum were sent from Northern Ireland to Scotland and foreign markets in the period 1933–37. This export trade in yarns has declined seriously since 1913 when over 9,000 tons were exported. There is also a small annual import of flax yarn into Northern Ireland amounting to about 2¼ thousand tons per annum on the average between 1933 and 1937. The corresponding figure for 1913 was approximately 7½ thousand tons.

No precise figures are available to show the extent to which the Northern Ireland linen industry is dependent upon foreign markets but as previously stated it has been estimated[4] that about 60 per cent. of the aggregate output is disposed of in markets outside the United Kingdom. It is quite certain, however, that the export trade of Northern Ireland with markets outside the United Kingdom has declined very substantially during the inter-war years. Thus, for example, United Kingdom exports of linen piece goods (the bulk of which are produced in Northern Ireland) declined from about 190 million square yards in 1913 to about 83 million square yards in 1937. Loss of such markets (to which the declining popularity of linen goods with the consuming public and foreign competition have both contributed) has been the factor mainly responsible for the decline in the production of linen piece goods in Northern Ireland.

In this connection it is interesting to note that although the depression of the early thirties was keenly felt by the linen industry there was a substantial revival both of the Ulster output, and the United Kingdom export, of linen piece goods between 1933 and 1937. In 1937 the export (by quantity) of linen piece goods from the United Kingdom was the highest since 1925 (with the exception of 1936) and the output of linen piece goods in Northern Ireland was comparable with that of 1924.[5] Thus from the point of view of physical quantity of production it would appear true to say that, although some bad years were experienced between 1924 and 1937, the Ulster linen industry has not permanently lost ground during this period.[6] From another point of view, however, the industry certainly did lose ground. The volume of employment declined by about 20 per cent. between 1924 and 1937 as the industry concentrated more on coarser yarns and on piece goods which were more easily and cheaply produced.

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[1] No figures are available for imports into and exports from Northern Ireland ince the Province forms part of the United Kingdom for foreign trade purposes. Figures given in this article for such imports and exports are based on shipments entering or leaving the port of Belfast which give a very close approximation. Unless the contrary is clearly stated such import and export figures include shipments to and from Great Britain as well as foreign markets.

[2] The bulk of which was grown in what has since become Northern Ireland. The terms Ulster and Northern Ireland are used synonymously in this article.

[3] This has been a factor of some importance in sustaining the coarse end of the trade.

[4] *Northern Ireland Census of Production*, 1935, p. 33.

[5] In 1937 the Ulster production of flax yarn was substantially in excess of that for 1924 and less than 5 per cent. below the 1912 level.

[6] In this respect its experience has been in sharp contrast with that of the cotton industry. British cotton piece goods production was 25 per cent. below the 1913 level in 1924, and 55 per cent. below the 1913 level in 1937. British cotton piece goods exports were 36 per cent. below the 1913 level in 1924 and 80 per cent. below the 1913 level in 1937.

Table III gives some idea of the structure of the two most important sections of the industry (spinning and weaving) prior to 1939. Details of numbers of spindles in place were obtained[1] in respect of 35 firms which controlled in the aggregate over 858,000 spindles—that is, all but a very small percentage of the total spindleage of the industry. All the firms concerned were engaged in the spinning of flax fibre but a number were also engaged to a varying degree in the spinning of hemp and ramie yarns. Details were also obtained from the same source of the number of looms in place in respect of 53 weaving firms which controlled in the aggregate about 26,000 looms—i.e., well over 90 per cent. of the estimated number of looms in the entire section. The firms were for the most part engaged solely in the weaving of linen and union piece goods but some of the firms also produced small quantities of cotton piece goods.

The table underestimates the extent to which control over productive capacity is concentrated in the hands of few firms. In certain cases firms which retain their legal identity are known to be controlled in effect by other firms, but for purposes of this table they have been treated as separate firms.[2] The number of such cases is small, however, though some of the larger firms are involved. The general tendency since 1914 has been for the large firms to become larger by absorbing the smaller firms. Few cases, if any, exist of small firms amalgamating with each other.

As a general rule each firm's spinning (or weaving) activities were confined to a single establishment but where a number of separate spinning (or weaving) establishments were operated by the same firm they have been treated as a single unit in Table III.

It will be noted that there is considerable uniformity of size of firm in the spinning section—26 of the firms operating between 10 and 30 thousand spindles. Nearly one-third of the firms operate between 15 and 20 thousand spindles. In spite of this general uniformity of size, seven of the largest firms (about 20 per cent. of the total) have an aggregate spindleage of about 350,000 or about 40 per cent. of the total spindleage of the industry.

There is the same tendency towards general uniformity of size in the weaving section—about 23 firms (or about 43 per cent. of the total number of firms) operate between 200 and 400 looms. These firms control in the aggregate nearly 6,500 looms or about 25 per cent. of the total capacity of the industry. On the other hand five large firms (with over 1,000 looms each) control nearly 30 per cent. of the total capacity and the seven largest firms in the section (with over 800 looms each) control about 36 per cent. of the total capacity.

It should be borne in mind in making comparisons that the most usual size of firm in the spinning section is a larger unit (from an employment point of view) than in the weaving section.[3] For example, we may take the modal unit in the spinning section to be a firm operating between 10 and 20 thousand spindles and the modal unit in the weaving section to be a firm operating between 200 and 400 looms. In each case modal firms would comprise 43 per cent. of the number of firms in the section. But such a modal unit in the spinning section would probably employ on the average twice as many persons at full capacity working as a modal unit in the weaving section.

Some account must also be taken of the presence of integrated firms, i.e., firms which are engaged in two or more sections of the industry. This factor has been neglected in the compilation of Table III—certain firms having been counted twice, once in each section.

Three large firms combine all sections of the industry—spinning, weaving, finishing, merchanting and making-up. These firms control about 14 per cent. of the entire capacity of the spinning section and about 18 per cent. of the weaving section. In addition two large firms of weavers, finishers, merchants, and makers-up, jointly control a fairly large spinning establishment although they compete with each other in the market with their manufactured linen goods. Also, at least one large weaving firm is known to control firms operating in all other sections of the industry.[1] In both cases the controlled firms retain their separate legal identity. If we include both these cases as fully integrated firms, combining all activities from flax spinning to the disposal of the made-up and finished product, it is probably true to say that about 26 per cent. of the spinning and about 29 per cent. of the weaving end of the industry is in the hands of fully integrated firms.

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[1] *British and Dominions Textile Industries Annual*, 1938. The details given apparently refer to 1937.

[2] In some cases the fact of complete control could not be definitely established. In another case a spinning firm *A* was jointly controlled by two weaving firms *B* and *C*. Since *B* and *C* remained active competitors with each other the firm *A* had to be treated as a separate firm for purposes of Table III. See below.

[3] It should be noted that number of looms is not always an accurate indication of size since they vary in pattern and productive capacity. It follows that the figures given below of volume of employment per firm can be only a rough approximation.

Apart from the above at least four large or medium-sized firms of spinners controlling about 10 per cent. of the total number of spindles also control about 10 per cent. of the total number of looms. Neither of these firms are engaged in finishing though some of them make-up and merchant their finished products.[2]

It is more difficult to estimate the degree of integration between weaving, finishing, merchanting, and making-up. For the most part the piece goods produced by the weavers are sold to merchants who send the goods to be finished on a commission basis, and then make them up in their own making-up departments. About one third of the total number of looms are in the hands of firms who do their own finishing but a very large proportion consist of those looms controlled by the fully integrated firms previously mentioned.

Association of weaving with merchanting and making-up is rather more common. No precise estimate can be given of the extent of this association but well over 40 per cent. of the total loom capacity is in the hands of weavers who do their own merchanting and making-up. Once again this is mostly accounted for by those firms previously mentioned which are either fully integrated or which combine all branches except finishing.

All this, however, should not be permitted to blur the over-all picture which is of a general specialisation of firms in one particular section. This is particularly marked in the spinning and finishing sections where nearly two-thirds of the work done is done by firms which confine their activities to one or other of these sections. In the case of weaving and making-up it is more difficult to get a clear picture but the non-weaving merchant is probably responsible for a larger proportion of the output of made-up goods than firms which combine weaving with merchanting and making-up.

During the inter-war years there has been a definite trend in the direction of more complete integration of the various sections and the increasing size of many firms has often been associated with an extension of the range of their activities. There is a strong feeling in the trade that, if the control of firms in other sections can be acquired on terms favourable to the parent concern, integration will lead to a lowering of the cost of production and a strengthening of the latter's competitive position. The strength of the arguments in favour of integration depend very much on the particular circumstances of each case and it certainly cannot be said that the most progressive and prosperous firms are all included in the small group of integrated concerns. Many large, efficient, and prosperous concerns are to be found which confine their activities to one section—this being particularly true of the spinning and finishing sections. Nor can it be said with certainty that all the fully integrated firms are shining examples of the advantages of integration. But one is left with the feeling that other things being equal[3] considerable advantages accrue to the integrated concern which are denied to the more specialised firm. Also that the future structural tendency of the industry will be in the direction of more extensive and complete integration. To some extent the present tendency in the direction of integration puts a premium on its further extension, as specialised firms find themselves catering for a more restricted market in the disposal of their products.[4] The advantages of integration are particularly stressed as between weaving on the one hand and merchanting and making-up on the other. It is frequently alleged that the large weaving firm without direct access to the market is in a very weak position and that in the past the independent merchants have ruthlessly played off such firms against each other when trade was depressed in order to force down prices. It is also maintained that weaving firms with their own selling organisations can often increase their profits (or minimise their losses) during periods of depressed trade by keeping the maximum number of looms running to full capacity and forcing the goods on to the market at very low prices.[1] The desirability of a link-up between weaving and finishing is less emphasised. Finishing is an extremely specialised process in which experience, skill, and technical knowledge are at a high premium. For this reason many weaving firms hesitate to enter this field though the efforts of the finishers to maintain standard charges has resulted in an increasing number overcoming their reluctance. As between spinning and weaving there is also some division of opinion as to the advantages of integration though the balance of opinion seems to be in favour of it. The desire to be assured of a supply of the right kinds and qualities of yarns appears to count for very little since it is generally agreed that the independent spinners cater adequately for the weavers in this respect. The fact that a spinning establishment can be better supported in time of depression if it is part of an integrated firm and the desire to eliminate the yarn merchant appear to be the chief explanations of the steady growth of integration between spinning and weaving.

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[1] Including three spinning firms which control in the aggregate about 80,000 spindles.

[2] In such cases the finishing of the piece goods is done by specialised independent finishing firms on a commission basis.

[3] i.e., Assuming equally efficient management, the opportunity to acquire sound concerns in other sections on reasonable terms, and a sufficiently flexible and efficient selling organisation to provide good access to markets.

[4] Since the outbreak of war one large fully integrated firm has further increased its size by taking over a medium-sized weaving firm.

In spite of the existence of a number of more or less integrated firms, the great majority of which are large firms, both the spinning and weaving sections are dominated by a comparatively large number of small or medium-sized firms which generally confine their activities to one section. In the past the large number of small firms in the industry has aroused much criticism. It has been alleged that for the most part they are uneconomic high cost units, since they exist mainly on small orders which give them a wide range and confine them to short runs. This criticism, however, is not always borne out by the facts since some of the firms cater for extremely limited but specialised markets which do not justify larger units. There is probably more force in this condemnation of the number of small firms when it is applied to the weaving section only. It is not likely that the degree of specialisation here justifies such a large number of very small firms. It is dangerous to attempt to generalise about the ideal size of the working unit in the linen industry but it is unlikely that, unless there is considerable specialisation, a single firm operating less than 400 looms will produce piece goods at the lowest possible cost. More than half the firms in the weaving section operate less than 400 looms. In the spinning section on the other hand, although many firms are on the small side the average size of the firm is much larger. The general consensus of opinion in the trade appears to be that no firm operating more than 10,000 spindles is necessarily a high cost producer. The point of maximum efficiency is probably somewhat higher than this but since only two firms (of those included in Table III) operate less than 10,000 spindles and only seven firms operate less than 15,000 spindles the existing structure of the spinning section is less open to serious criticism.

There is, nevertheless, a strong case for larger firms in the spinning section as well as in the weaving section.[2] Whilst there is no reason why a firm operating more than 400 looms or 10,000 spindles should not be technically efficient there are other factors to be taken into account. Low manufacturing costs will not save a firm from charges of inefficiency if the best available raw material for the job in hand is not purchased at the lowest possible price, if the best prices are not obtained for the product, if selling costs are not kept down to the minimum, and if the general administrative side of the business is not efficiently organised. It is in these respects that the larger firms frequently show up to advantage. These firms can generally command more working capital, can purchase in greater lots and are often in a better position to take immediate advantage of a favourable market situation to strike good bargains in the purchase of raw materials. This is all very much related to the even more important factor of management.[3] It is the larger firms which offer the greatest scope and can offer the highest rewards for first-rate managerial ability and this often gives them a great advantage over the small firm. Variation in managerial ability appears to be the chief explanation of the considerable variation in efficiency between different firms in the industry.[4]

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[1] It would not necessarily follow that such firms would be selling below their costs though the price might be below the costs of competing firms which were working very much below capacity. This may partly explain past allegations (and denials) of weak selling on the part of apparently prosperous and highly efficient firms.

[2] It is assumed that market conditions permit all firms to work to near full capacity. Otherwise the case for larger units is much weakened. See below.

[3] By management we mean the general control and supervision of a business as distinct from detailed technical supervision of working operations. Both functions may be combined in the same person in a small firm.

[4] In conversation with the present writer one authority stated that there was sometimes a variation of 33⅓ per cent. in terms of output per loom between firms working on the same type of goods.

Although it is probably true to say that the general level of efficiency in the industry is not low, such variation between firms suggests that it is not high (particularly in the weaving section) and that there is considerable scope for reduction in the general level of costs. It seems probable that such reduction could best be secured by further enlargement of the size of firms and by further progress in the linking up of firms between the different sections. It is not suggested that in practice the large integrated firms are necessarily the most efficient. This is almost certainly not the case. But it is these firms in most cases which have the greatest opportunities to achieve maximum efficiency. There can be no guarantee that in practice advantage will be taken of these opportunities.[1]

Under these circumstances it is important to enquire into the reasons why so many small and non-integrated firms have contrived to linger on despite the generally adverse market conditions experienced between the two wars.[2] Part of the explanation no doubt is that many of them are " country " firms which have been able to pay lower wages than those ruling in the vicinity of Belfast. It is also sometimes alleged that some of these firms have made a habit of turning out slightly inferior goods which are nevertheless good enough to deceive buyers on the look-out for bargains, but this is difficult to verify. In some cases small firms have been handed down from father to son, and their present owners are content to work with a very small return to their labour and capital rather than abandon a business which is their sole and only likely source of income. Many " weak sellers "[3] are to be found amongst these firms. Weak selling on a falling market has been a marked feature of the industry during the inter-war years and though it offers short-term advantages to consumers it is to be deplored when it is associated with exploitation of labour and the undermining of capital values elsewhere in the industry. The industry is not without some justification when it claims some protection against weak selling. But this is a matter to be proceeded with cautiously lest other abuses (e.g., subsidisation of inefficiency and exploitation of the consumer) develop.

The large number of small and medium-sized firms in both spinning and weaving sections, together with the tendency of firms to confine themselves to a single section, has had a further consequence. It has meant that no single firm or small group of firms has been in a position to dominate the industry or any section of it and to give a lead in the direction of co-operative industrial re-organisation.[4] That is to say, no Ulster linen firm has been in a position to play the part played by the Lancashire Cotton Corporation in furthering the re-organisation of the cotton industry. The limited extent of integration between the sections has added to the difficulty of doing anything along these lines. Disharmony produced by the competing rivalries of a large number of small firms within each section has been added to by the conflicting interests of the various sections. Any substantial measure of agreement on common measures for the rehabilitation of the industry has therefore been impossible to attain. Mainly for these reasons no progress has been made in the direction taken during recent years by many important industries in Great Britain, e.g., shipbuilding, coal, iron and steel, and cotton. Up to the outbreak of war in 1939 the linen industry probably remained the closest of all British industries to the free competitive ideals of the 19th century.

The principal trade organisations in the industry before the war were The Flax Spinners' Association, Ltd., The Irish Power Loom Manufacturers' Association, The Irish Linen Merchants' Association, and The Handkerchief and Embroidery Association. For the most part these associations were merely employers' organisations without regulatory functions, confining themselves to discussing questions of common interest, making representations to Government Departments, and entering into negotiations with trade union organisations on wages questions, etc. Occasional efforts have been made by some of these organisations to arrive at and maintain minimum price agreements and to exert some control over output. Up to the outbreak of war, however, these efforts had met with little or no success.

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[1] Thus, for example, an old-established family concern may become inefficient not through any structural defect (it may be both large and fully integrated) but because management is kept within the family despite the fact that the present generation exhibits no conspicuous degree of managerial ability.

[2] Adverse market conditions to some extent have favoured the small firm. Overhead charges bear particularly heavily on the large firm if it is forced to contract output.

[3] That is, firms which sell below the real cost of production. They can rarely be positively identified even where their existence is almost certainly known. But it would not be true to say that only small firms have been weak sellers in the past.

[4] E.g., Co-operative measures designed to strengthen control over the market by central selling, central allocation of orders, price control, output control, etc., or to secure an increase in efficiency by promoting amalgamation schemes, " through-ticket " schemes, or schemes for the elimination of redundant plant.

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The most serious effect of the present war on the Ulster Linen Industry has been to produce an acute shortage of its basic raw material—flax. It is mainly dependent in peacetime on flax imported from Russia, the Baltic, and the Low Countries but since 1940 imports from these sources have been negligible. To meet this shortage every effort has been made to increase the production of flax in Northern Ireland. Between 1939 and 1943 the acreage under flax in Northern Ireland increased from 21,000 to over 90,000. The yield per acre has varied from year to year but the tonnage of fibre produced is estimated to have varied from about 9,000 tons in 1940 to nearly 17,000 tons in 1943. That is to say, about 40 per cent. of the normal flax requirements of the industry were met or could have been met by Ulster-produced flax in 1943. A small proportion of this Ulster flax, however, is known to have been sent to Great Britain but this has probably been more than offset by the greatly increased Eire production of flax the bulk of which is imported into Ulster for use by the Ulster linen industry. Although no official figures are published it is probable that the supply of flax available to the industry since 1940 has varied between 30 per cent. and 45 per cent. of its normal pre-war requirements—approaching, and possibly exceeding the latter figure in 1943.[1]

To provide for the retting and scutching of the increased home production of flax the Government has found it necessary to build additional flax processing factories and to provide funds for extending some factories which existed in 1939. The new factories have been leased to private enterprise for the duration of the war. Private enterprise could not have been induced to provide additional processing facilities without some guarantee that the volume of Ulster flax production after the war would be maintained at wartime levels.

To encourage home production of flax the Government has been forced to offer prices substantially in excess of the market prices ruling before the war. Increased reliance on home-produced flax has therefore meant a substantial increase in costs and prices of linen goods. Other factors have contributed to the rise in linen prices but the high cost of flax has probably been the most important single influence. In this connection it is worth noting that, during the early years of the war when considerable efforts were being made to expand exports, there were repeated complaints that export business was difficult to get because linen prices were too high. It was also stated that American consumers were buying rayon and cotton goods in preference to linen because of the high price of the latter.[2] The sensitivity of foreign demand for Ulster linen to price considerations suggests that the present inflated home production of flax is not likely to continue after the war unless a Government subsidy is forthcoming.

Apart altogether from the shortage of raw material, wartime activity in the trade has been further restricted by Government limitations placed on the home market. By the Limitation of Supplies Orders deliveries of linen goods for consumption by the civilian population have been restricted since 1940 to 20 per cent.—25 per cent. of the pre-war quantity.[3] These Orders have chiefly applied to the movement of goods from stock since manufacture for the home market (otherwise than on Government account) has been prohibited since November, 1939. Releases of flax are made, however, and permits to spin and weave granted,[4] when it could be shown that the goods were required for export to " hard currency " countries. At first these restrictions bore heavily upon the industry and resulted in a large volume of unemployment. The steady growth in volume of Government bulk orders for textile goods has since relieved the position a good deal. These orders did not affect all sections equally however[4] and the spinning section in particular is still only partly employed as a result of Government restrictions and the shortage of raw materials.

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[1] Since 1939 the production of flax in Great Britain has considerably increased and increased imports have been received from Canada, New Zealand, Australia, Egypt, and Argentine. Some flax has been received by Northern Ireland spinners from these sources but no figures are available.

[2] *Vide*, *Linen Trade Circular*, March 21st and May 9th, 1940.

[3] Cotton and rayon were more favourably treated under the Limitation of Supplies Orders. Wartime restrictions may therefore have forced upon the public a substitution of cotton and rayon for linen goods. This may have its effect on the post-war demand for linen.

[4] See below.

It is difficult to estimate the extent to which the capacity of the industry is likely to be permanently affected by wartime idleness. Many firms which have temporarily closed owing to lack of trade may be expected to resume operations after the present emergency has passed and if market conditions permit. In attempting to estimate the immediate post-war capacity of the industry some account must also be taken of the number of looms and spindles destroyed in the air raids on Belfast in 1941. Also a number of spinning and weaving factories have been taken over for war production purposes, the textile machinery being dismantled and stored or in some cases scrapped. On the whole it is probable that between 5 per cent. and 10 per cent. of the pre-war spindles and looms will not be available to the industry after the war.

One of the most important effects of the war on the industry (and one which may considerably affect its post-war development) is the change which has been effected in the type of product. Since 1940 large Government orders have been placed with the industry but these have been mainly, though not wholly, for coarse linen goods, linen and hemp goods, cotton goods, or cotton and linen unions.[1] Some of these goods had not previously been produced by the industry.[2] In May, 1942, it was estimated[3] that about 900 tons of cotton yarn per month was being imported into Northern Ireland, i.e., about three times the normal pre-war quantity. Taking into account the fact that the weaving section has not been working to full capacity, this suggests that at least 40 per cent. of the raw material used by the weaving section during the past two years has been cotton.

It follows from this that the weaving section has been less badly hit by the war than the spinning section since it has become less dependent on flax which has been in very short supply. Since the beginning of 1942 the weaving section has worked steadily to about 70 per cent. of normal peacetime working. During the same period the spinning section has been working between 30 per cent. and 40 per cent. of normal peacetime working. Even this low rate of working showed some improvement over the early years of the war when the flax shortage was more acute. This wartime depression has not been evenly spread over the spinning section since tow spinning machinery has often been extremely active whilst line spinning machinery has for the most part been lying idle.[4]

In the face of this situation some spinners have turned their attention to rayon, and appreciable quantities of 100 per cent. rayon staple yarn have been spun on orthodox flax spinning machinery. It has been reported that the weavers have produced some attractive goods with this yarn but at present this wartime development is hardly beyond the experimental stage.

The linen thread section has so far been kept fairly busy on Government work but the making-up trade at first suffered severely from restrictions placed on the home (civilian) trade. Since 1941, however, the position of the making-up trade has been eased by large orders on Government account. Here also there has been a considerable change over to work on cotton and union goods and the trade has had to adapt itself to the making-up of goods not previously handled (e.g., haversacks).

The industry has been subject to Government control since September, 1939, when the Flax Control Board (acting on behalf of the Ministry of Supply) was set up. An immediate inventory of all flax and yarns in stock or in process was taken and further transactions in flax and yarn were prohibited without the consent of the Board. No person was allowed to acquire, spin, weave, or subject to any process, any flax in the United Kingdom except under licence from the Ministry of Supply. No person was to dispose of flax except to a person producing a licence from the Ministry of Supply authorising him to acquire that flax. All flax straw, fibre, and tow, was in future to be offered to the Ministry of Supply.

In practice releases of flax and licences to spin and weave have only been granted in order to fulfil Government contracts and to meet export orders for hard currency countries. Block Government contracts for cloth are generally placed with the Irish Power Loom Manufacturers' Association which allocates the orders equitably amongst weaving firms who are prepared to undertake the work. For this purpose the Association has established a Clearing House known as the Government Contracting Section which is also responsible for seeing that the quality of the cloth is kept up to the required specification. To meet the yarn requirements of the weavers engaged on such Government work releases of flax are made by the Control Board to a Committee of the Flax Spinners' Association which distributes it amongst spinning firms on the basis of pre-war standards of flax consumption.[1] Government contracts for made-up goods are generally placed by the Ministry of Supply (acting through the Northern Ireland Ministry of Commerce) with a committee containing trade representatives working under the ægis of the Ministry of Commerce which allocates the orders between the different firms.

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[1] i.e., Tent ducks, dowlas cloth, canvas, denims, cotton shirts and drawers, etc.

[2] E.g., Dowlas or "blitz" cloth generally used for temporarily re-roofing blitzed houses.

[3] *Linen Trade Circular*, May 23rd, 1942.

[4] Generally speaking tow yarn is used for weaving medium and coarse goods whilst line yarn is used for the finer linen products.

Since 1941 the Ministry of Commerce in co-operation with the Flax Control Board has operated a scheme for the control and direction of the linen export trade. The scheme makes provision for Government supervision of the execution of export orders in order to ensure that raw materials released for this purpose would not find their way in the form of manufactured goods on to the home market.

This wartime control machinery has much in common with that imposed on other British industries but it has perhaps a particular significance in the linen industry. It has given the leading trade associations a new importance and also considerable experience of industrial regulation and control which they formerly lacked. They will emerge from the war much strengthened in prestige, better equipped with the necessary knowledge and experience, and therefore more ready to continue exercising similar functions in peacetime. In other words the way seems to have been prepared for the linen industry to follow the course of " industrial self-Government " pursued by so many other British industries during the thirties.[2]

TABLE I

PRODUCTION, CONSUMPTION OF RAW MATERIALS, AND EMPLOYMENT IN THE ULSTER LINEN INDUSTRY IN 1935[3]

Spinning Section	Number of workpeople employed[4]	24,800
	Production of flax yarns	28,850 tons
	Consumption of flax	44,348 tons
Weaving Section	Number of workpeople employed[4]	20,000
	Production of linen and union piece goods (loom state)	million sq. yards 146
	Consumption of flax yarns	24,050 tons
Linen Thread	Production of linen thread	1,450 tons
	Consumption of flax yarns	1,800 tons
Finishing Section	Number of workpeople employed[4]	6,000
	Linen and union piece goods printed, dyed, bleached and/or finished	million sq. yards 123.6
	Linen yarn bleached, dyed, printed and finished	13,200 tons
Making-up	Number of persons employed[4]	10,800
	Linen and union piece goods handled	million sq. yards 65.1

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[1] This method of spreading-over the available Government work was particularly uneconomical in the spinning section (which has been working much below full capacity) and it has not always been followed. For example, in June, 1942, allocations of flax to spinners to meet a Government contract for aero linen were made directly by the Control and were not evenly spread over the whole spinning section. It was explained that by concentrating the allocation in the hands of certain spinning firms, better yarns could be spun at lower cost and with less waste than by spreading it over the entire section.

[2] It is perhaps significant of this associative tendency produced by wartime developments that in 1942 a new Association (Yarn Merchants' Association) was formed which now handles all the collective yarn orders placed by the Ministry of Supply.

[3] Source: *Northern Ireland Census of Production*, 1935.

[4] Figures given for workpeople employed in each section exclude outworkers. The total number of outworkers employed in 1935 was about 1,500. The employment figure given for the Spinning Section includes persons employed in linen thread manufacture.

TABLE II

PRODUCTION OF FLAX YARNS, LINEN THREAD, AND LINEN PIECE GOODS IN NORTHERN IRELAND IN VARIOUS YEARS 1912—1937

Year	Production of Flax Yarns tons	Production of Linen Thread tons	Production of linen and union piece goods (loom-state) million square yards
1912	32,650	—	222.0[1]
1924	28,050	2,060	160.6
1930	20,050	1,550	116.3
1933	27,250	1,400	141.8
1934	31,550	1,400	157.9
1935	28,850	1,450	146.0
1937	31,350	1,475	161.5

[1] ESTIMATED. The figure returned for the 1912 Census of Production was 228 million linear yards. Figures for 1912 relate to the whole of Ireland.

MAIN SOURCE: Census of Production (Northern Ireland) 1930 and 1935. Figures for 1933, 1934, and 1937 were collected for the purposes of Import Duties Act Inquiries. The figures for 1933 and 1934 were reproduced in the 1935 Census of Production report. The figures for 1937 were supplied to the present writer by the Northern Ireland Ministry of Commerce.

NOTE: The figures given in the above table are not strictly comparable for the various years but the error involved in making comparisons is slight except as between 1912 and subsequent years.

TABLE III

SIZE OF FIRMS IN THE SPINNING AND WEAVING SECTIONS OF THE ULSTER LINEN INDUSTRY (1937–38)

Spinning Section		*Weaving Section*	
Number of Spindles	Number of Firms	Number of Looms	Number of Firms
50,000 and over ..	2	1,999—1,000	5
49,999—40,000 ..	3	999—750	3
39,999—30,000 ..	2	749—500	10
29,999—20,000 ..	11	499—400	6
19,999—10,000 ..	15	399—300	11
Under 10,000 ..	2	299—200	12
Total number of Firms	35	199—100	5
Number of spindles accounted for ..	858,000 (approx.)	Under 100	1
		Total number of Firms ..	53
		Number of looms accounted for ..	26,000 (approx.)

History and Theory in Economics

By F. A. Lutz

The following pages contain a short report on an important book in the field of economics, published in 1940 in Germany under the title *Die Grundlagen der Nationaloekonomie*.[1] Its author is Professor W. Eucken. The date and place of publication explain why it has escaped the attention of economists in Anglo-Saxon countries. It apparently gave rise to a good deal of discussion in German periodicals, it was out of print in 1941, a second revised edition was published in 1942 and a third one in 1943. The present writer is unfortunately not in possession of these later editions, nor has he access to the periodicals which devoted articles to the book.[2] The presentation which follows is based on the first edition.[3]

In an understandable fear that his book may be laid aside as just another methodological treatise, the author goes so far as to deny in the preface that the book he has written is a methodological study, and indeed it is very different from the standard type of treatise on methodology. The reader will not find lengthy abstract discussions of the place of the " inductive " or " deductive " method in economics, or of the problem whether the method of economics is related to that of the natural sciences, or whether economics is a *Geisteswissenschaft*, and similar topics which usually take up so much space in methodological literature.

But in spite of the author's denial the book is in part a treatise on methodology. Taking economic reality as we observe it to-day, or as it was in the past, as his starting point, and never losing sight of it, the author develops the economic problems posed by this reality and gives a coherent answer to the question of what method the solution of these problems requires. He does more than this, however; he shows by means of illustrations how economic reality can be explained by the application of his methodology. The strong feeling for economic reality, the thorough knowledge of economic history, the belief that the economist has to penetrate it in all its manifestations in order to grasp the nature of the problems with which he has to deal, the conviction that the appropriate method of procedure in economics is dictated by the character of economic problems and cannot be found by abstract methodological discussion, the application of the methodology, all this gives the book a characteristic stamp distinguishing it from other methodological studies.

The observation of everyday economic life leads the economist to pose the familiar problems of (1) the allocation of resources between the various wants; (2) the determination of the " time " structure of production; (3) the choice of the technical method to be applied; (4) the distribution of the product; and (5) the location of industry: five problems which are actually only five aspects of the same problem since, being interdependent, they can only be solved simultaneously by general equilibrium analysis. In addition, we have the problem of economic dynamics; i.e., of the change of economic activity as it occurs over time along these five lines.

Everyday economic activity is, however, part of the general historical development that is going on at the time, and we know from economic history that the problems mentioned above have been solved in different ways in different periods. It appears, therefore, that the historical method has to be used in economics if we are to understand a given period. Only with the background of a full knowledge of the political and social environment of a period, i.e., as part of universal history, can we hope to understand economic events, which after all are only one aspect of the general historical development at any given time. How are we to understand the economic history of the Middle Ages without knowing what were the political structure and religious ideas of the time? Or Germany's economic development in the last ten years without similar knowledge? Economic life, just as the other spheres of life, has that character of historical uniqueness which led the historians to develop their own peculiar scientific method. There is no escape from this conclusion.

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[1] Gustav Fischer, Jena, 300 pp.

[2] Except for the article of H. von Stackelberg, " Die Grundlagen der Nationaloekonomie ", *Weltwirtschaftliches Archiv*, 1940, pp. 245–81.

[3] Some of Professor Eucken's articles should be mentioned as supplementary material, in particular: " Die Ueberwindung des Historismus " in *Schmoller's Jahrbuch fuer Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich*, 1938, pp. 214 ff., and " Wissenschaft im Stile Schmoller's ", *Weltwirtschaftliches Archiv*, 1940, pp. 468 ff.

On the other hand, it is equally clear that the economic problems presented above cannot be solved by the historical method, since no amount of observation and description of facts, of intuition and of feeling for historical atmosphere, enables us to explain the interrelationships within an economic system; e.g., it is not possible from the observation of facts alone to find out the effect of the introduction of new machines on employment, wages, consumption, etc. At every moment so many things occur in economic reality that mere observation cannot enable us to trace the observed facts to their causes. We have to dissect the events which we observe into their components, to create in this way theoretical models which help us to find the effect of a particular change which we isolate from all others, in order to discover the interrelationships which mere observation does not divulge.

This dual nature of economic problems (i.e., their historical character on the one hand, the necessity of theoretical treatment on the other) is the starting point for Eucken's investigation. It is the same problem which was at stake in the famous discussion between Schmoller and Menger.

The author prepares his answer by reviewing various schools of economic thought from this particular standpoint.

The classical school was not fully aware of this dual character of economic problems. It would be wrong to say, as is so often said, that the classical thinkers had no understanding for the historical character of economic events. Even a superficial glance at Adam Smith's *Wealth of Nations* shows the contrary. But what the classical school of economics, a child of the period of enlightenment, looked for in history was not the unique character of institutions, nations, personalities and events, which was the object of study of nineteenth century historians. They were interested in the natural order and in natural laws, and they studied history in order to discover behind the manifold and constantly changing variety of forms they found there the " natural " order which they believed to exist. In the economic sphere they found it to be an economic order which was based on competition, and they proceeded to analyse this order. This attitude made them largely indifferent to the process of historical development, and their system, therefore, does not reflect the variety of historical phenomena in the economic sphere of life.

The discredit into which the classical school fell, its doctrinairism and the feeling that its teachings did not correspond to reality, led economists to approach their science along new lines.

One group, particularly in Germany, started out with a discussion of the concepts used in economics, an analysis of the " nature " of " production ", the " nature " of " exchange ", etc., as the foundation for their theories. The last representative of this school is Othmar Spann. In an excellent critique of this *Begriffsrealismus*, Eucken likens them to the mediæval monks who in mid-winter discussed the question whether milk freezes and tried to find the answer by analysing the nature of milk without its ever occurring to them to put milk into the open air and see what happened. He shows the philosophical origin of this procedure, discusses its logical character (definitions have to be formulated at the end of scientific analysis not at the beginning) and makes it clear that it is bound to lose all contact with reality and to lead to the formation of sects in economics, furiously fighting each other.

A second approach is connected with the name of Carl Menger (and, as regards the broader problem of the relation of history and the social sciences to natural sciences, with the names of the philosophers Windelband and Rickert). These writers preached a dualism of method. History (and therefore also economic history) is concerned with concrete, unique phenomena; theory, with what is general in those phenomena. As Menger said, economic history " aims at the understanding of concrete phenomena in their specific context ", whereas theory tries to discover the " laws which lie behind the succession of economic events " (*Gesetze ihrer Aufeinanderfolge*). This split of economics into two sciences, one of a historical, the other of a theoretical character, has dominated economics ever since, and has given rise to two branches of economics which have little to do with each other and neither of which, by itself, can solve the problems which economic reality poses. The theorist very often lost the feeling for the challenge which economic reality threw out to him and became absorbed in refinements of the theory, while the economic historian merely described the events without much attempt to analyse them. Despite repeated assertions by the representatives of the historical school that they would finally arrive at a general theory by first collecting and describing as many facts as possible, they never did, and never could succeed in so doing, because it is impossible to construct theory through description. The dualism of the economic problem cannot be overcome in this way.

Finally there is a last attempt to solve the problem, again due mainly to German economists, who seem to have been intrigued more than the economists of other countries by this dual nature of economics. This attempt is connected with the names of List, Knies, Roscher, Schmoller, Buecher, and Sombart. These men tried to discover stages of economic development which would reflect the essential features of various periods in history (mediæval economy, capitalism, etc.). And they advocate that for each of these historical stages a separate economic theory should be constructed and the dualism of economics thus overcome. The theory which applies to a specific historical period will then describe the economic reality of that period. (It is therefore called *Anschauliche Theorie.*) The discrepancy between economic history and theory thus seems to be removed by the simple method of forming *Zeitgebundene Theorien ;* i.e., theories each of which describes a specific period, and does not apply to any other period.

Eucken shows that the stages of economic history as they were developed by these men are historically wrong, that each historical period contains a variety of forces, many of which have to be overlooked in an attempt to construct a theory explaining that period; that consequently the theory never gives an adequate picture of the period,[1] and that, most important of all, it is a complete misunderstanding of the logical character of theory to think that it can describe reality. For this reason this last attempt to overcome the dilemma is also repudiated by Eucken.

What does he offer in its place?

Economic activity in any historical period we may choose, was and is carried on within a *Wirtschaftsordnung*, an order of the economic society, and is only intelligible within the framework of this " order ". Most of these *Wirtschaftsordnungen* are the result of historical growth, they are not thought out rationally by men and then put into practice. There are exceptions to this rule, the most important being the competitive order which was planned and its working analysed by the early classical school before it was established, and which was introduced partly as a result of their work. The first task of the economist who aims at the explanation of reality is, therefore, to discover the structural elements of the economic systems (*Wirtschaftsordnungen*) which have prevailed in economic history. Eucken exemplifies his method by giving detailed historical illustrations from the past and the present without trying to take off the edges of the individual phenomena by concentrating attention on what may be " general " in them. This investigation leads him to conclude that, in spite of the infinite variety of forms within which man has seemingly carried on his economic activity, there is only a limited number of fundamental structural elements; and the variety of forms which we seem to observe in economic history stems from the fact that these structural elements can be and have been combined in different ways, just as the twenty-six letters of the alphabet, combined in different ways, make a great variety of words and languages possible.

The knowledge of these " ideal types ", which are gained from the detailed examination of historical facts by a method of abstraction which Eucken describes at some length, opens the road to the solution of the fundamental dilemma of economics.

Since economic activity by its very nature cannot be carried on without planning, Eucken forms his ideal types by investigating the nature of economic plans and the way they come about. This gives him immediately the two fundamental types of a centrally planned economy on the one hand, and an economic system in which the plans are made by individuals (households and firms) on the other. The first type is subdivided by him into four forms, all of which have existed (though usually mixed with elements of the second type) in economic history; space does not, however, permit our going into these four forms. In the second type of economic organisation, the problem of the co-ordination of individual plans arises, and is solved in different ways according to the marketing situations which prevail. In this connection Eucken draws up an exhaustive list of the marketing situations which he thinks have prevailed at one time or another in the course of economic history. Here we shall only mention that he first distinguishes two groups—" open " and " closed " marketing situations—the first prevailing when there is free entrance into the trade (which is by no means exclusively a phenomenon of the nineteenth century), the second when the entrance is closed, as in the case of certain guilds of the Middle Ages where new craftsmen were not allowed to establish themselves, or the case where, as in recent times in some countries, additional investments are forbidden even to firms already in the industry. It is clear that the latter case does not necessarily constitute oligopoly or monopoly since it is compatible with the existence of hundreds of firms competing with each other. Indeed all marketing situations are possible within both the " open " and the " closed " groups. Eucken presents his results in a table which contains five possible marketing situations on the supply as well as on the demand side, giving twenty-five combinations in all. Since each of these can be either " open " or " closed ", the final number of forms is still greater. In addition we have price fixing by the Government which can be applied in any one of these situations. In describing his marketing situations, Eucken is not as " purist " as the mathematical economists. He does not hesitate to speak of free competition even in the case of non-homogeneous products, since, so he maintains, the individual firm, even though it does not produce *exactly* the same commodity as others, in many cases takes the price as given, i.e., does not follow any price policy of its own.[1] Since the co-ordination of the plans of individuals takes place in different ways in different marketing situations, all such situations have to be investigated. Into his discussion of economic systems and of marketing situations, the author weaves an interesting analysis of the various forms of economic power.

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[1] It is particularly interesting to read what the author has to say about the stage of " capitalism ", what varieties of form are hidden behind this term which makes it quite impossible to construct a " theory " for the period of capitalism and what disastrous consequences the personification of this term (" Capitalism has done this and that ") had for economics. In this respect, as in some others, Eucken's book has many points of contact with Hayek's views, as expressed in " Scientism and the Study of Society ", Part II, ECONOMICA, February, 1943.

In a similar fashion Eucken investigates the monetary systems in the framework of which economic activity is carried on. He again finds different " ideal types " which he distinguishes according to the method by which money comes into existence, e.g., whether by way of the creation of debt claims, or by vesting a certain commodity with the functions of money, etc. Here too he illustrates from economic history. The creation of money can be, and has been, carried out in different marketing situations. We find examples in this field of monopoly, oligopoly (as when towns in the Middle Ages competed with each other to expand the circulation of their coins in each other's districts), free competition and other marketing situations.

Eucken distils his ideal types from the observation of economic history. This gives the impression (whether intended or not by the author is not quite clear) that an intelligent man without a knowledge of economic theory, could establish these ideal types by studying economic history. But it seems to the present writer that the ability to classify these types in an intelligent way already presupposes some knowledge of economic theory. We can classify the monetary systems in the way Eucken does only because we know that the economic process develops differently within the different monetary framework which he distinguishes. Although Eucken's ideal types doubtless have to be abstracted from economic reality, economic theory helps us to define them. Theory has a rôle to play even at this first stage of the economist's task, the discovery of ideal types through the close observation of reality.

After the limited number of ideal types of organisational framework have been established, the main task of the economic theorist begins, i.e., to analyse the fundamental economic problem (in its five aspects), and the process of economic change, within each of these possible frameworks. The theoretical apparatus thus constructed does not describe the concrete reality of any historical period (it is not *Zeitgebundene Theorie*), it is a system of theoretical models of timeless truth.

The work already done by theoretical economists has all contributed to the building up of this apparatus, although in some cases they either have not been aware that their theories were dependent on specific assumptions about the organisational framework and institutional setting, or else have believed that their theories explained a specific historical epoch, such as capitalism or " the " business cycle of the capitalist period, and so on.

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[1] Eucken, in a passage on cartels, shows how the latter always knew how to make use for their own purposes of the prevalent ideologies of their time. Monopolists justified their existence in periods when people believed in the " natural law " by applying this idea to their own case; in times of *laissez-faire* by pointing out that the formula implied freedom to organise cartels; in times (such as the 'twenties in Germany) when there was talk of " economic democracy ", by claiming that they were the true representatives of that democracy; and in times of socialistic tendencies by maintaining that they were leading the way to socialism. He does not doubt that they will also turn to their advantage the conclusion of economists that free competition and monopoly are two extreme cases of marketing situations to which nothing corresponds in reality, where all that we have are different degrees of monopolistic power, a conclusion which renders authoritative action against monopolies more difficult.

None of the models are, of course, worked out in detail by Eucken. What he does at some length is to analyse the nature of economic plans in the various economic systems that are distinguished by him. The reason for his taking up this particular point is that the nature of economic plans constitutes the criterion for his distinction between the two fundamental organisational frameworks (" planned " economy and " free " economy). He shows how they are dependent on non-economic data (which he discusses in detail) and empirical " laws " (such as the " law of diminishing returns "); how the plans of the individuals in a free system differ from the plans of a " dictator " (the latter basing them directly on these non-economic data and empirical laws, while the former base them not only on these but also on prices which however, in turn, depend on the constellation of these ultimate data and, therefore, project the latter indirectly into the plans of the individual); how the co-ordination of the individual plans is achieved in the " free " system, and how through divergences between what he calls the " planning " data and the " factual " data the element of risk finds its place in the theoretical apparatus. He insists that for the ideal type of a " free " economy the analysis must start with the plans of individual households and firms. He would not approve of the type of economic analysis which proceeds solely in terms of " aggregate " concepts (such as " saving ", " investment ", " national income ").

Concerning the problem of economic change, Eucken maintains that it cannot be explained by a theory of " the " business cycle. Here, too, all that we can do is to develop theoretical models which show how a change in any one of the data (including political action) in the economy affects the economic process over time. Eucken thinks that the historical evidence shows that " business cycles " are very dissimilar, one from another. Moreover his conviction that it is a misconception of the logical character of theory to think that it " describes " reality, confirms him in his belief that no general theory of the business cycle can ever offer an explanation of historical economic development, i.e., of a succession of concrete and dated events.

The working out of dynamic models by way of " process analysis " has occupied economists increasingly during the last ten years. Eucken does not go into the difficulties which are raised by this type of analysis. Since it requires assumptions as to " what happens first ", i.e., as to the reaction times of the various elements (e.g., the reaction time which it takes for the supply of a commodity to respond to an increased demand), and since the analysis follows a different course according to the assumption we make in this respect, we should end up with literally an infinite number of dynamic models. Moreover, it is not possible to discover the reaction times from the observation of the facts, and even if it were, there would be no guarantee that they would still be the same in the future as they were at the time of the investigation.

Equipped with these theoretical models constructed for the various ideal types of economic organisation, the economist can then return to reality (from which he started) and, by their application, explain economic reality *everywhere and at all times.* Not all parts of the theoretical apparatus are relevant (although they are all true) for the explanation of a given period of economic history. For some periods some models are less important than others; the investigation of the facts of the period will have to indicate which of them have to be applied and in what combination. The author again gives concrete examples of how this can be done.

The assertion often made by economic historians that the economic attitude of man has changed in the course of history, is also examined. Eucken's conclusion is, in brief, that there are certain patterns of behaviour which can easily be brought into a theoretical scheme. Changes in the attitude of man, therefore, do not prevent us from developing theoretical models of timeless truth.

To recapitulate: Starting from the investigation of economic reality as it is now and has been in the past, the economist first has to establish the ideal types of possible economic systems and their structural elements (such as the marketing situations or the monetary systems), develop theoretical models analysing the economic process within each of these possible frameworks and then, equipped with this knowledge, return to economic reality in order to explain it. This procedure is the only way in which it can be explained; the economic historian cannot rely on a separate " historical method " to solve his problems. He can solve them only by applying the tools with which the theorist supplies him.

Book Reviews

The Road to Serfdom. By F. A. Hayek. London: George Routledge & Sons, Ltd. 184 pp. 10s. 6d.

Professor Hayek tells us in his Preface that " this is a political book ", but though he devotes a good deal of space to topics which are more commonly treated by political scientists than by economists, he is eminently within the scope of political economy in the wider (and legitimate) sense of the term. He has written a challenging and provocative book, and one that should be read above all by those who are most likely to disagree with his general position. Indeed it is dedicated with some irony " To the socialists of all Parties ".

The author, himself originally an Austrian economist, is profoundly disturbed by the trend of thought in this country as to the future of our economic and social system, and he draws ominous parallels with what he has observed on the Continent of Europe. He sees even the liberals here abdicating from their traditional rôle of guardians of individual liberties, while the socialists and the whole of the intellectual left-wing seem to have swallowed Planning, hook, line and sinker. In order to be fair to Professor Hayek it is necessary (though he himself makes it by no means easy) to remember continually a warning which he explicitly makes: " It is of the utmost importance to the argument of this book for the reader to keep in mind that the planning against which all our criticism is directed is solely the planning against competition—the planning which is to be substituted for competition. This is the more important as we cannot, within the scope of this book, enter into a discussion of the very necessary planning which is required to make competition as effective and beneficial as possible ".

Many people would agree with Professor Hayek that *total* planning of the economic system would involve the loss of the greater part of the liberties which the average Englishman takes for granted, and which he would rise in his wrath to defend if he thought they were seriously threatened. The fact that throughout his argument he assumes a very comprehensive scale of planning, has the advantage for him that he can pick his ground where it is the strongest and most favourable for the case he wishes to make, but it also deprives him of much support which he might otherwise have gained. We feel he is trying to frighten us with a bogey, when what we want is practical advice and counsel. The British people do not, in fact, believe in hard and fast logical solutions; they much prefer to compromise, even at the cost of some illogicality. What kind and degree of State control is necessary for and compatible with the efficient working of the economic system, with its acceptance by those who live and work under it, and with the maintenance of our essential liberties? These are the kind of questions which are being asked at the present time and on these Professor Hayek is deliberately silent. He would seem also to be unduly obsessed by the example of Continental countries with the result that he fails to make sufficient allowance for the Englishman's deeply-rooted love of local and decentralised initiative.

His statement of the case for the competitive system, especially from the point of view of liberty, is one of the best that has ever been put forward. In particular, Chapter VII, entitled " Economic Control and Totalitarianism " is a brilliant piece of reasoning. But if the competitive system has large virtues it also has important defects and the author is not so convincing in dealing with these. When he argues that the economic disorders of the inter-war period were not the result of the working of the economic system, which had no chance to function properly since it was hampered and impeded on every side, he is open to the answer that the underlying cause of the disorder was the failure of effective demand. Even though the measures which Governments in their desperation adopted may have been unwise and may have aggravated the situation, it yet remains true that the competitive system broke down at one of its vital points—the maintenance of the flow of money incomes, and all the other evils followed from this. Again, when he treats monopoly as the outcome of the wrong sort of State action and harks back to nineteenth century conditions as those to which we can return if we only possess the determination to do so, he seems very remote from the politico-economic realities of the contemporary world. The big aggregations of economic power have come to stay, and the problem is how to find means of bringing these *imperia in imperio* to a position in which they serve the interests of the community and do not threaten to dominate it. It may be true, as he points out, that even taken together they have far less autocratic authority than a State, which is in the position of being the sole employing body, would have. But the fact remains that they do possess irresponsible power in a measure which is felt to be incompatible with the maintenance of a democracy. Finally, Professor Hayek glosses much too lightly over the abuses which inevitably arise in a system where the main incentive and determinant is profit-making. His own antithesis between planning *for* and planning *against* competition does not meet the point, because the issue cannot rightly be presented in these terms.

To conclude, this is a courageous and utterly sincere book by an able thinker. No one who reads it will deny the right of Professor Hayek to say to the world of the future: " You have been warned ".

C. W. Guillebaud.

The National Income of Jamaica 1942. *The National Income of Barbados 1942*. Being Nos. 5 and 9 respectively in the series of Development and Welfare Bulletins relating to the West Indies. By FREDERIC BENHAM. 1944. 29 pp. and 24 pp. Each 10 cents. Obtainable from the Advocate Company, Ltd., Bridgetown, Barbados.

These memoranda provide most valuable information on the national income of two West Indian territories. They are extremely short, and apart from a brief discussion of the concepts employed are entirely devoted to setting out the details of the estimates with notes on the constituent items which are arranged in tables. Dr. Benham as Economic Adviser to the Comptroller has had opportunities for collecting and assessing material which would not be open to an unofficial investigator. He is exceedingly forthright about the reliability of the different components and in this respect the works are a model for other investigators even though the statements on accuracy are necessarily couched in broad qualitative terms. It may be thought that Dr. Benham takes an unduly optimistic view of the accuracy of his final figures for he claims that the estimate of the national income of Barbados is probably correct within 1 per cent., but this in itself is a refreshing change from those investigators who attach to their estimates margins of error so wide that they would, if taken seriously, destroy the value of most of the work to which they apply.

In fact, so far as can be judged from the evidence presented, the final totals do appear to be fairly reliable. They are arrived at by an ingenious use of data which enables the most to be made of the statistical information available. The approach on which the main reliance is placed is described as the production method but as the following demonstration will show it could perhaps more properly be regarded as a modification of the usual expenditure approach. In setting out the method adopted the treatment of Barbados will be taken as a model since this is the later study and is not in all respects identical with the treatment of Jamaica.

The usual approach via expenditure involves the measurement of the following items :—

(1) Consumers' expenditures at market prices.
(2) Net home investment at market prices.
(3) Expenditure by public authorities on goods and services at market prices.
(4) Foreign lending (overseas investment) or, alternatively, the excess of current receipts from abroad over current payments abroad
less
(5) The excess of indirect taxes over subsidies.

Dr. Benham's main modifications of this scheme are to restrict items (1) to (3) to locally produced goods and therefore to measure exports of goods and services together with receipts from abroad from presents and business interests in place of (4). Further the items are not all presented exactly as shown above so that certain adjustments have to be made *en bloc* at the end. The resulting scheme is as follows :—

(1) Exports of domestic produce including the increase in stocks awaiting export ;
(2) Locally produced foodstuffs for local consumption at retail prices ;
(3) Locally produced manufactures, public utility services, etc., for local consumption, excluding certain producers' goods included in the value of other goods, at retail prices ;
(4) Government expenditure on goods, services and subsidies other than those re-sold to the public (post office, waterworks) or shown elsewhere as part of new construction;
(5) Value added to imports for final consumption in handling them locally ;
(6) Services not elsewhere included ;
(7) Gross rentals including rates ;
(8) New construction ;
less
(9) Imported materials for industry, agriculture, etc., including duty ;
(10) Duplication of local handling charges in imports and purchases of services which are mainly consumer services by business firms and public authorities.

The sum of these items gives a measure of locally produced output and to it is added

(11) Net current receipts from overseas lending, presents from abroad and exported services not included above.

Two points may be made. First, rates are included in (7) but nowhere deducted being in fact treated as direct taxes. This is unusual and not commented on. It may be that these property taxes are fundamentally different from their counterpart in this country. Second, in the study of Jamaica, renewals and replacements are deducted from a list substantially similar to the above. This appears to be a misconception and is not followed in dealing with Barbados. It is clear that income derived from such construction is part of the national income though, of course, the profits of firms whose equipment is worn out in production are reckoned after deducting these allowances from their receipts. Dr. Benham's approach clearly requires that no deduction be made since these values appear but once in home production. Had he shown gross and not net construction under (8) the position would, of course, have been different.

The categories set out above each contain a considerable number of separate items and indeed the detail in which it has been found possible to estimate expenditure, may come as a surprise to many readers. In view of the considerable detail available it is disappointing that

Dr. Benham has restricted himself to rather rough comparisons of this method with calculations of income and outlay and does not provide any other accounts such as the income and expenditure accounts of public authorities or the balance of payments for which most but not all the elements are provided. As a result the work tails off a bit at this point both methodologically and as a statement of the structure of the economies studied. For example, while considerable information is provided about the expenditure of public authorities divided between transfer payments, and goods, services and subsidies, no statement is given of the revenue and borrowing from which this expenditure was financed. Another instance can be seen in trying to construct a balance of payments table. In the following table only transactions mentioned by Dr. Benham are shown: it may be that a complete statement would be affected by other transactions not mentioned here.

OVERSEAS TRANSACTIONS OF BARBADOS IN 1942
(£'000)

Exports		Imports (table 17)	1374
goods (table 18)	1013		
Services (table 11, items 3 + 4)	66		
Remittances from		Foreign lending	y
Barbadians abroad (table 11, item 2)	250		
Net interest	58		
Local sales to tourists (table 11, item 1 less item 8)	x		
	$1387 + x$		$1374 + y$

Foreign lending on this showing (and as stated above there may be many omissions) amounts to $13 + x$. This provides an element necessary to estimate the uses of domestic saving which may be set out as follows :—

	£'000
New construction (table 8)	70
Increase in stocks awaiting export (table 1, item 13)	713
Foreign lending	$13 + x$
	$796 + x$

This compares with an estimate of £880,000 for private saving reached in the study by deducting current outgoings from income. It seems clear that the difference could easily be reconciled although this cannot be done with the information provided.

These estimates are greatly to be welcomed both for the information they provide on the structure of two West Indian economies and the methods used in turning the statistical material available to the purposes of this sort of calculation. It is of interest to note that a similar calculation is promised for St. Vincent though it has not yet reached this country. It is greatly to be hoped that steps will be taken to keep these estimates up to date and that the results will be published as soon as is practicable. Finally, I hope that Dr. Benham will, as time permits, cast his estimates in the form of balancing accounts and round off these statements with a calculation of the other main social accounts discussed above.

RICHARD STONE.

Studies in Economic Dynamics. By M. KALECKI. George Allen & Unwin, Ltd. 1943. 92 pp. 6s.

The author's gift of producing, with extreme economy of argument, an elegant and fruitful theory, and straightway confirming it with statistical evidence, gives to these essays a quality which one may almost call dramatic. This impression is strongest perhaps in the chapter on the Short-Term and the Long-Term Rate of Interest, where data covering the relation of these rates for nearly a century are shown to be explainable by no more than two linear functions, and where the one or two points which do not fall on these regression lines actually add force to the argument because in each case there is a special and powerful circumstance explaining and requiring such a discrepancy, so that here indeed the exception does prove the rule. What is more, Mr. Kalecki is able to show that if the regression co-efficients at which he arrives were, say, half of the value actually obtained, some of the co-efficients in his theoretical equation (equation (4) on page 39) would assume absurd values. Thus the statistical test to which he has submitted his hypothesis is one which could have disproved it, had it been wrong in certain respects : but, on the contrary, the hypothesis passes this test with striking success. There is, perhaps, some necessary arbitrariness in the division of the whole period into intervals ; and this may have allowed an extra good fit to be obtained. But this does not prevent the essay as a whole from carrying conviction. It might perhaps have been worth while to justify the procedure (on page 34) of scaling-down the financial circulation, by pointing out that, since the marginal convenience of holding cash is presumably the same, at the same rate of interest, in financial and in non-financial balances, therefore equal changes of marginal convenience are likely to be produced by *equi-proportional* changes of velocity of circulation in the two circulations.

I have dealt first with what seems to me the most characteristic of Mr. Kalecki's essays: but the others are of no less importance. In the one called *Costs and Prices* he is able to show that on simple assumptions the relative share of wages in the product of industry, in conditions of less than full utilisation of plant, depends mainly on the degree of market imperfection and oligopoly which prevails in the industry. The statistical material here, while very interesting, is not quite so strikingly convincing as in the essay on the rates of interest.

No doubt the main secret of Mr. Kalecki's power to obtain highly interesting and substantial theoretical results with such directness and economy is his skill in devising very powerful yet acceptable simplifying assumptions. The remaining three essays all illustrate this, and since there is not space to deal with all in detail, I will devote what remains to the essay on the " Pure " Business Cycle.

Mr. Kalecki's tool for analysing the business cycle is his fundamental equation, given as equation (11) on page 69. This equation is the mathematical description of his business cycle model. It is arrived at by setting up a number of equations each expressing one of the *direct* psychological links in the economic causal mechanism, e.g., expressing capitalists' current consumption as a function of their profits, i.e., incomes, of a specified earlier date; or the current rate of investment decisions as a function of the rates, at a specified earlier date, of increase or decrease of profit, growth of total fixed equipment, and flow of entrepreneurs' saving; and so forth; and then eliminating between these equations so as ultimately to obtain a differential equation in only one variable, but involving values and derivatives of this variable at several different dates. The variable chosen is investment, and in the equation ultimately obtained, the *direct* psychological links have disappeared and their place has been taken by the mediated causal connections, namely those between the levels and rates of growth or of acceleration of growth of investment at earlier and later dates. Much ingenuity in the choice of assumptions and their embodiment in symbols is concealed in the smooth march of the elimination process by which the fundamental equation is finally attained; but though this appearance of ease and the clearness of exposition tempt one to read quickly on, it is very well worth the reader's while, as a form of mental discipline and for the sake of the æsthetic pleasure to be had from taking in the whole ingenious construction at one viewing, to retrace his steps from time to time until he can build up the fundamental equation step by step almost from memory, in a rapid turning over of the pages.

In order to assess the relevance, as distinct from the beauty, of the model, it is necessary to consider very carefully just what the various assumptions involve. I have no reason whatever to suppose any of them unrealistic, but it might have been worth while to support by some statistical evidence or further theoretical discussion the assumption on page 63 that $\eta\delta < 1$, and the statement on pages 66, 67 that " the combined time lag $\omega + \theta$ is rather small (something like a year) ".

When we come to watch the actual working of the model, attention has to be directed mainly to the co-efficient a, and this turns out to be in fact a box containing a lot of rather intricate mechanism, and much of the essential working of the model goes on inside this box. It may be felt by some readers that the co-efficient a is rather heavily loaded with the considerable collection of various factors and influences which it has to take care of; but to my mind Mr. Kalecki has rightly stopped the further elaboration of his algebra at the point where it would have begun to hamper and obscure his analysis rather than help it. One feature in his account of the behaviour of this co-efficient does seem to me doubtfully valid: he says on page 73: " But then the entrepreneurs have not much of the boom behind them and therefore a does not fall strongly . . ." This reasoning credits the entrepreneurs with an awareness of the cycle and a confidence in its regularity which can perhaps hardly be attributed to them before 1930, and could even now be forgotten, or ignored by a new generation. A last point concerns the co-efficient e which connects the rate of change of working capital and stocks with the rate of change of profits. The author suggests in the footnote to page 79 that while in a boom the value of e must eventually fall, in the slump there is nothing to bring its value down and thus ensure that the slump will level off and be reversed. But is there not here also an ultimate arresting influence in that neither stocks nor working capital can fall below zero, so that e must here again ultimately fall to a very low level?

Any economist who is asked " What can economic theory do by way of explaining actual concrete facts? " would do well to point out these essays.

G. L. S. SHACKLE.

The Nazi Economic System. Germany's Mobilisation for War. By OTTO NATHAN. Duke University Press. ix + 378 pp. $4.00.

How Nazi Germany Has Controlled Business. By L. HAMBURGER. Brookings Institution, Washington, D.C., U.S.A. 1943. Faber & Faber, London. 1944. 101 pp. 5s.

Dr. Nathan, as a former high official in the German Ministry of Economics, is well qualified for the task he has set himself in this book. He has previously dealt with sectional aspects of his present subject in a number of articles in the American economic journals. His book is a monument of painstaking scholarship. The standard of accuracy is amazing. A detailed index helps to make the book into a standard work of reference. Here is the trustworthy " What's what " for the economic Germany of 1933—1939.

It is interesting to see that such a close observer does not accept the widespread view that the German system was based on peaceful " work creation " until about 1935 and then changed over to war preparation. Dr. Nathan finds the clue to the system in war preparation from the very beginning in 1933. The present reviewer is inclined to agree with him.

The author has adopted the principle of relying, on the whole, on the primary German material. In doing so, he does perhaps less than justice to the work of his predecessors, like Guillebaud, Sweezy, Neumann, Poole, or especially the pioneer work of R. A. Brady on " The Spirit and Structure of German Fascism " which is not even mentioned. One would have liked to know how far Dr. Nathan agrees with their conclusions.

Economic students are bound to notice a tendency to describe legal forms and administrative arrangements rather than economic or political contents. For instance, to mention the institution of Reich Defence Commissioners (on p. 51) without explaining that these are identical with the Party *Gauleiter*, is rather like Hamlet without the Prince of Denmark. It is only fair to say, however, that the tendency to formalism is partly implied in the decision to deal with methods and techniques rather than results. For another part it is due to secrecy and the lack of reliable information of the actual day-to-day working and real influence of the various organisations and bodies. The full story, as Dr. Nathan himself agrees, will have to wait until information can be obtained from the persons who have watched (or even operated) the system from the inside, and until such inside observers are free to give their full critical views.

The story of Foreign Exchange control and its spreading tentacles as unfolded in the book is worth pondering by all those with " Schachtian " hankerings. It is also most instructive to see how the system maintained traditional forms of organisation while completely inverting their true purpose (e.g., employers' organisation from a pressure group into an instrument of State control). This fact, in itself, should serve as a warning against a too formalistic approach.

In a volume so comprehensive there are inevitably some things with which a reviewer would disagree. It seems that Dr. Nathan tends to overestimate the power of the German commercial banks before 1933. In particular, I think he underestimates the tremendous influence of the memory of the 1923 inflation on economic policy, and for this reason misses the clue to wage policy which he correspondingly attributes to purely political or anti-consumption motives. In particular, if the memory of 1923 had been so completely displaced by confidence in the direct controls (as Dr. Nathan explains)—why then was the Mark not devalued which would have avoided so many troubles ?

The book also deals with wartime developments though in a more sketchy manner. The wartime section is a reminder how fast times move. Although Dr. Nathan's data are remarkably up-to-date (the sources used extend well into 1943), a good deal of his wartime analysis, such as the use of cartels as government agencies or the powers of the Ministry of Economics are already quite obsolete.

Let us make it quite clear that whoever wants to study the 1933–39 period in Germany, simply cannot afford to by-pass this book.

The second book (sponsored by the Brookings Institution) is much less substantial, in every respect, than Dr. Nathan's. The thesis which the book sets out to prove, according to the fly-leaf is that " Although National Socialism has posed as the protector of the German business community and the interests of investors, it has . . . so curbed and directed the conduct of business that the men in charge of private enterprise are deprived of all real freedom of action ". This thesis which will make the American business man's flesh creep evidently contains but one aspect of a complex truth ; it would not be impossible to find some support for the exactly opposite view.

Otherwise the book seems to fall between two stools. As a reference book, it disqualifies by its slightness, the absence of an index and an often surprising arrangement of the matter. (Standardisation, for instance, is dealt with in the section on " Control of Supply of Materials "). For an analytical treatment, on the other hand, there is not enough interpretation or even co-ordination of the detailed facts and measures. The reviewer would also question some of the author's descriptions, such as the one of the tax-free depreciation accounts which Mr. Hamburger considers as sheer robbery. There is, however, some colourful detail in the book. One learns that the Bremen export merchants once received the Minister of Economics during a war-time in a room in which the various forms needed for export licences were pinned up and competely obscured the wall paper. There is little doubt that the Manchester merchants could stage a similar reception for Dr. Dalton !

H. W. Singer.

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